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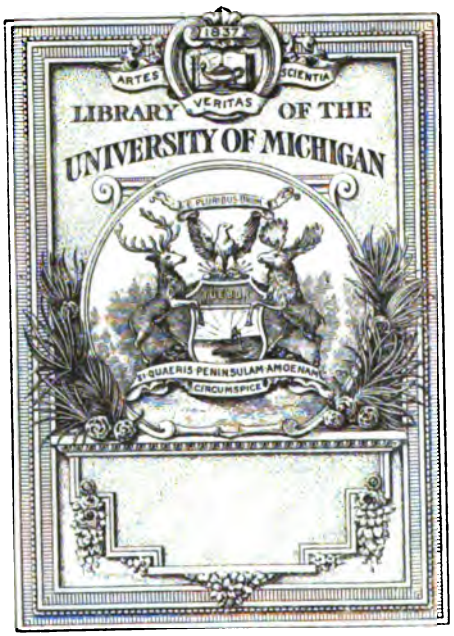
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TRANSACTIONS
OF THE
AMERICAN CERAMIC SOCIETY

CONTAINING THE PAPERS AND DISCUSSIONS OF THE FOURTH ANNUAL MEETING HELD AT CLEVELAND, OHIO FEBRUARY 10-11-12, 1902

PUBLISHED BY THE SOCIETY
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OCTOBER 1902

TRANSACTIONS

OF THE

AMERICAN CERAMIC SOCIETY

CONTAINING THE PAPERS AND DISCUSSIONS

119492

OF THE

THIRD ANNUAL MEETING

HELD AT

OLD POINT COMFORT, VIRGINIA

FEBRUARY 11-13, 1901

The Society is not, as an organization, responsible for the statements of fact or opinion expressed in its papers and discussions.

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CONTENTS.

Page

Officers of the Society for the year 1901 5

Membership of the Society 6

Rules 10

Preface to Volume III 15

PAPERS.

1. The Fineness of Flint, in its Relation to the Manufacture of Pottery Bodies 17
By STANLEY G. BURT, Cincinnati, Ohio.

2. Hydrated Silicic Acid in Kaolin, and its Effect on Pottery Bodies 25
By WILLIAM H. ZIMMER, PH. D., Coburg, Germany.

3. A Method for the Rational Analysis of Clays for Cement Manufacture 46
By ALBERT V. BLEININGER, B. Sc., Columbus, Ohio.

4. The Classification and Nomenclature of the Silicates 65
By PROF. EDWARD ORTON, JR., E. M., Columbus, Ohio.

5. On the Permissible Variation in the Silica and Alumina Contents of Raw Lead Glazes 82
By WALTER M. FICKES, E. M. (in Ceramics), Carnegie, Pa.

6. A New Glaze-Flux 113
By WILLIAM CANNAN, JR., Syracuse, N. Y.

7. The Birth of English Porcelain 142
By PROFESSOR CHARLES F. BINNS, Alfred, N. Y.

8. The Use of Glazed Clay-Ware as an Architectural Decoration in Exterior vs. Interior Work 158
By OTTO HENSEL, Chicago Heights, Ills.

9. The American Ceramic Society in its Relations to the Practical Potter 159
By EDWIN C. STOVER, Trenton, N. J.

10. Unweathered vs. Weathered Clay 171
By EARL McK. OGLE, Indianapolis, Ind.

Contents — Continued.

11. Report of the Committee on Seger Cones..... 180
By ADOLPH F. HOTTINGER, Chicago, Ills.

12. Further Notes on the Distribution of Cost in the Manufacture of White-ware..... 193
By ERNEST MAYER, Beaver Falls, Pa.

13. Further Notes on the Tensile Strength of Clay Mixtures 198
By PROF. EDWARD ORTON, JR., E. M., Columbus, Ohio.

DISCUSSIONS ON ANNOUNCED TOPICS.

14. Defects of the Continuous Kiln of Today, and Prospective Improvements..... 205
15. Relation between Kiln Atmosphere and Color of Porcelain.. 216
16. Standard Methods of Clay Testing..... 217
17. Crystalized Kaolinite..... 219
18. Burning White-ware with Hard vs. Soft Coal 221

MISCELLANEOUS.

19. In Memoriam—William Golding..... 225
20. President Binns' Remarks on the Occasion of the Elevation of the First Associates to Full Membership..... 227

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FOR THE YEAR ENDING FEBRUARY, 1902.

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RULES.

OBJECTS.

The objects of the American Ceramic Society are to promote the arts and sciences connected with Ceramics by means of meetings for social intercourse, for the reading and discussion of professional papers, and for the publication of professional literature.

MEMBERSHIP.

The Society shall consist of Honorary Members, Members and Associates.

Honorary Members must be persons of acknowledged professional eminence, and shall not exceed five in number.

Members shall be persons competent to fill responsible positions in Ceramics and have suitable qualifications.

Associates shall include persons interested in Ceramics and the allied arts.

Honorary Members shall be proposed by at least five Members, approved by the Council, and receive at least 90 per cent. of the votes cast by letter ballot at the annual meeting.

Members and Associates shall be proposed by at least three Members or Associates, approved by the Council, and receive at least 75 per cent. of the votes cast by letter ballot. A candidate for admission must make application on a form prepared by the Council which shall contain a written statement of his age, professional experience, and that he will, if elected, conform to the laws, rules, and requirements of the Society.

All Honorary Members, Members, and Associates shall be equally entitled to the privileges of membership, except that only Members shall be entitled to hold office and to vote. Applicants for a change in grade of membership shall conform to the requirements of a new applicant.

Any person can be stricken from the membership of the Society on the request of five or more Members, on the recommendation of a majority of the Council, if he fails to resign on the advice of the Council. Such person, however, shall first be notified of the charges against him, and be given a reasonable time to appear before the Council, or present a written defense, before final action is taken by the Council.

DUES.

Honorary Members shall be exempt from all dues.

The initiation fee of Members shall be ten dollars, and of Associates five dollars, which if not paid within six months after election, will render the election void.

The annual dues for Members will be fixed by the Council, but shall not exceed five dollars per year.

The annual dues for Associates will be fixed by the Council, but shall not exceed four dollars per year.

Any Member or Associate in arrears for over one year may be suspended from membership by the Council until such arrears are paid.

OFFICERS.

The affairs of the Society shall be managed by a Council, consisting of a President, Vice President, Secretary, Treasurer, and three Managers, who shall be elected from the members at the annual meeting, and hold office until the adjournment of the meeting at which their successors are elected.

The President, Vice President, Secretary and Treasurer shall be elected for one year, and the Managers for three years; and no President, Vice President, or Manager shall be eligible for immediate re-election to the same office.

The duties of all officers shall be such as usually appertain to their offices, or may be delegated to them by the Council or the Society; and the Council m at its discretion require bonds to be furnished by the Treasure

Vacancies in any office shall be filled by appointment by the Council, but the new incumbent shall not thereby be rendered ineligible to re-election at the next annual meeting to the same office. On the failure of any officer to execute his duties within a reasonable time, the Council, after duly warning such officer, may declare the office vacant, and appoint a new incumbent.

A majority of the Council shall constitute a quorum; but the Council shall be permitted to carry on such business as it may desire by letter.

ELECTIONS.

At the annual meeting, a nominating committee of five Members, not officers of the Society, shall be appointed, and this committee shall send the names of nominees to the Secretary at least 60 days before the annual meeting, who shall immediately forward the same to the Members. Any other five members may also present the names of any candidates to the Secretary, provided it is done at least 30 days before the annual meeting. The names of all candidates, provided their assent has been obtained, shall be placed on the ballot without distinction as to nomination by the regular or an independent nominating committee, which shall be mailed to every member, not in arrears, at least 20 days before the annual meeting.

The ballot shall be inclosed in an inner blank envelope, and the outer envelope shall be endorsed by the voter, and mailed to the Secretary for collection. The blank envelopes shall be opened by three Scrutineers appointed by the Chair at the annual meeting, who will report the result of the election. A plurality of the votes cast shall elect.

MEETINGS.

The annual meetings shall take place at such time and place as the Council may decide, at which time reports shall be made by the Council, Treasurer, and Scrutineers of election, and the accounts of the Treasurer audited by a committee of three appointed by the Chair.

Other meetings may be held at such times and places during the year as the Council may decide, but at least 20 days' notice shall be given of such meetings.

Seven Members shall constitute a quorum at any regular meeting, and a majority shall rule, except where otherwise specified.

The order of business at the annual meeting shall be.—

1. Reading of Minutes of last meeting.
2. Reports of the Council and Treasurer of the Society.
3. Announcement of Election of Members.
4. Announcement of Election of Officers.
5. Appointment of Nominating Committee.
6. Old Business.
7. New Business.
8. Reading of Papers.

PUBLICATIONS.

The Council shall act as a Publication Committee, and decide as to what to publish. The publications of the Society shall be sent to all Members and Associates not in arrears. The Secretary will furnish each author with reprints of his papers at cost price, provided due notice is given that reprints are desired.

The Society is not, as a body, responsible for the statements or opinions expressed in its publications, papers or discussions.

PARLIAMENTARY STANDARD.

Roberts' "Rules of Order" shall be the parliamentary standard on all points not covered by these rules.

AMENDMENTS.

These rules may be amended at any regular meeting by a two-thirds vote of a letter ballot at the subsequent annual meeting provided a written notice of such proposed change is sent to each Member at least 30 days before said annual meeting. Said proposed amendments shall be printed on the ballot for officers and counted by the same Scrutineers.

PREFACE.

The following pages contain the papers and discussions of the Third Annual Meeting, held at Old Point Comfort, Virginia, February 11th, 12th and 13th, 1901.

This volume comprises the record of the society's most profitable years work. That of last year contained the Manual of Ceramic Calculation, of over eighty pages, and assumed fictitious size on that account. The contributions of the present year, while not more numerous, are longer, and, it is hoped, more thorough and of a higher grade technically than the earlier work of the society.

Although it is not the policy of the society to publish a record of its business meetings, this custom has been over-ridden in one instance on page 227 of this volume. The remarks of President Binns on the election of the first full members from the list of the associates, seemed not only to be peculiarly impressive and felicitous, but also to define in words, a principle which has so imbued the members of the society as to have the force of an unwritten law. For these reasons, it is herein reproduced, and it is hoped that it may recall a memorable scene to those who witnessed it, and inspire in those who did not, a fuller realization of the opportunities for individual growth and development which the society affords.

The growth of the society has continued most rapid. In spite of the fact of the loss of six names from its rolls, the present list shows the flattering increase of over forty per cent., which is certainly as large a proportion as any organization can assimilate and imbue with its own principles and ideals.

For the first time in our short career, we are called upon to chronicle the loss of one of our members by death. The memorial to MR. WILLIAM GOLDING appears on page 225.

The previous publications of this society are comprised in Volumes I and II, and The Manual of Ceramic Calculation. The latter is included in Volume II, or can be purchased separately at the price of $1.00. The price of Volumes I and II is $4.00 each to persons not members of the society, while members can obtain extra copies at 50 cents and $2.00 respectively until further notice.

Volume III will be sent promptly on publication to all members not in arrears; extra copies will be sold to members at the price of $1.00 per copy, and $4.00 to persons not members.

Please address all correspondence for the purchase of publications to STANLEY G. BURT, Treasurer, Rookwood Pottery, Cincinnati, Ohio.

EDWARD ORTON, JR.,
Secretary.

COLUMBUS, O., August 1, 1901.

THE FINENESS OF FLINT IN ITS RELATION TO THE MANUFACTURE OF POTTERY BODIES.

BY

STANLEY G. BURT.

The word Flint, originally of a limited meaning, is now used to cover all types of silica furnished to the potter. In nature, two main varieties occur, the crystalline and the amorphous, either of which may analyze 99 per cent. or over pure SiO_2. Of the crystalline, two kinds are regularly sold, the rock and sand flints, both in the main from the same source. The rock, we to-day mine and grind; the sand, nature, in decomposing the rock, mined for us and partially ground. Flint pebbles, the true original flint, found largely on the sea-shore, especially in France, gives us the amorphous variety, sold by dealers as French flint. Silica is also found of apparently an intermediate character, between the crystalline and the amorphous.

The question now arises, can any one of these types be substituted for the other? It seems incredible, that so important a point has not been settled long ago, but to-day one potter will tell you that rock goes a great deal farther, that is, you need less of it; while another is perfectly satisfied with sand. It is my purpose to give in this paper, the results of a few experiments I have lately carried out, endeavoring to throw a little light on this question. First, however, I purpose to give a brief review of the subject.

In the use of flint in bodies and glazes, there are many points to be considered. Here, however, I wish to merely consider flint in its bearing on the question of crazing. To have a perfect glaze we must have a uniform co-efficient of expansion and contraction, for glaze and body. Expansion and contraction should in no way be confused with shrink-

age. A clay body when fired loses water and shrinks. Increased heat gradually fuses the clay, and the body continues to shrink. That is, the clay piece undergoes a permanent decrease of size. This is the shrinkage of the body, and *per se* has nothing to do with its co-efficient of expansion and contraction. It is true that increased heat gives increased shrinkage, and at the same time a constantly changing co-efficient of expansion, but these are are separate and distinct phenomena.

I have often heard it stated by potters, that a vitrified body would craze where a porous one would not, the physical condition of vitrification, being in their minds the cause of the craze. The fact that the vitrified body would craze, might often be true, but this result would be due, *first*, to the fact that from a porous body, the glaze could absorb much more silica, and *second*, to the fact, that in a vitrified body, the silica has passed from the free crystalline state, to the combined amorphous condition, and would not be due, as they would have us believe, to the fact that a porous body permitted the glaze to enter the pores, hold better and not craze. How does an enamel get into the pores of iron?

If we take a piece of pottery in our hands, the heat from the hand immediateiy causes the clay body and glaze on it to expand. The amount of expansion for one degree of heat, represents the co-efficient of expansion and of contraction on cooling. Each separate ingredient, entering into the composition of the glaze or body, has its own individual co-efficient, which either raises or lowers the co-efficient of the mixture.

In the body, pure kaolin has a low co-efficient, that of spar is also low, but flint has a high one; so Seger gave us the law "to increase the co-efficient of the body, add flint." In a glaze, the case is reversed, lead and most of the bases, have a high co-efficient, compared to which the flint is low; so Seger says, "to decrease the co-efficient of the glaze, add flint." In further explanation of these two laws, we now know, that the crystalline, free, flint in the body, has a higher co-efficient than the amorphous combined flint, in the glaze.

With these two laws, how plain is the difference between the composition of china and porcelain! Both start with pure kaolin for the body, with its low co-efficient; with porcelain, we wish to vitrify this, so we must add spar, also of low co efficient, and can add but little flint, to raise the total co-efficient. We now have to balance up by adding a great deal of flint to the glaze, and thus bringing the glaze co-efficient down. With china we wish to use a softer glaze containing some lead, and consequently having a high co-efficient; so here we give up the vitrification of the body and add to it all the flint it will carry, to bring its co-efficient up to the lead glaze.

Before leaving the co-efficient question, let me give another example. In making dishes for boiling acids, not only must we aim for a body proof against the action of acids, but we must also provide a body which can stand sudden changes from heat to cold. Obviously a body with high co-efficient will not do; so we leave out just as much flint as possible, replacing it with the low co-efficient aluminum silicate, that is, kaolin.

Before passing on to Seger's next law about flint, I wish to explain, that when I speak of pure kaolin, having a low co-efficient, by pure, I mean at least 95 per cent aluminum silicate. We call any short clay, which burns pure white and stands a high fire, a kaolin, but it would be a mistake to say that a kaolin, showing say 40 per cent. free silica, has a low co-efficient. The rational analysis must always be kept in mind. Any one, for instance, who takes Seger's rule, substitutes a ball clay for a kaolin, to increase the co-efficient, and replaces a kaolin showing 40 per cent free silica, with a ball clay showing but 10, will probably come to grief.

Seger's third rule about flint is, "the finer the flint, the greater the increase in the co-efficient of the body." This rule acts with greater force than either of the other two, that is, a slight variation in fineness will have more effect than a slight increase of flint, in body or glaze. I have seen it stated that one-third less flint very fine, served as well as the full charge not fine. The importance of the fineness of

the flint cannot be over-estimated. Unless we have a fine grain, there can be no true separation of the clay particles, we have lumps of flint and lumps of clay, each with its own co-efficient. It is only with the complete mix obtained from fine grains that we get the benefit of the high flint co-efficient. Furthermore, a glaze when completely fused takes up flint from the body; now not only does a glaze made with fine flint fuse much more readily, but the finer the flint in the body, the more of it will be taken up by the glaze.

In considering now, why it was that one could use this flint and not that, and vice versa, there seemed but two explanations: *First*, that it all rested upon the question of finenes. This I believe the true explanation. If I am using, say 30 per cent of rock flint, all of which passes a No. 14 lawn and substitute sand flint, which leaves say an 8 per cent. residue on the lawn, even though I allow for this loss, the sand flint which passes the No. 14 lawn, would not be as fine as the rock and craze might result. The same would be true reversing the flints. *Second*, that it rests on the difference in coefficient between crystalline and amorphous flint. How would it be, if, using the crystalline rock, I substituted for it the amorphous French flint. We know that the co-efficient of the amorphous silica is not as high as that of the crystalline. Therefore, would a body using the rock flint and exactly suited to a glaze, show craze if we changed to a French flint of the same fineness of grain? This might be possible, and might also offer an explanation, of why an overfired buscuit piece should craze, for we have in it our free flint combined and no longer crystalline. The vitrified body, thus produced, allowing of less absorption of silica by the glaze, is an additional factor.

Coming now to the experiments, the points to be shown are, *first*, with a uniform fineness of grain, is there any difference between rock and sand flint, in a body or glaze, and *second*, with the same fineness, can French flint be directly substituted for either of these? It must always be borne in mind, that fortunately for us, glazes have a certain elasticity, allowing what I call a margin of safety. Between craze on the one hand and peeling on the other, there is considerable

range, within which, the glaze is all right. On this subject of the margin of safety, I hope at some future date, to have a paper for the Society, showing whether it is greater with an increased body co-efficient, or a decreased glaze co-efficient. What I wish to call your attention to here, is that unless there is a pronounced difference between the co-efficient of the amorphous and the crystalline silica in the body, a variation might not be noticeable.

For testing the various flints, I got samples from different dealers and first examined their fineness of grain. I passed each through a No. 14 lawn, then through a No. 16 lawn and found an astonishing difference, some showed as high as 15.8 per cent. residue on the No. 16 lawn, while others showed but 0.5. per cent. As my time was too limited to arrange an apparatus by which I could get an absolute uniformity in size of grain, I decided to grind all, so as to pass the No. 16 lawn.

Two other factors should be considered here, *first*, wet or dry grinding, and *second*, calcining. While I am not a microscopist and am open to conviction of my error, my firm belief is that in this connection it is immaterial whether we grind wet or dry; it all depends upon the result obtained, that is, the fineness of grain of our product, and that it is the fineness and not the shape of the particle, which affects the co-efficient.

As for calcining, theoretically I can see no ground for its causing any change in the flint, still it is an interesting point, which a future series of trials will have to determine. In the samples I experimented with, the rock and French flints were calcined, the sand was not.

Five samples were selected. Number one and number two were American rock flints, number three a French flint and numbers four and five, sand flint. The last sample was washed from a fine white kaolin, and was furnished without grinding. On the lawns, these gave the following results:

Number of the Sample	Derivation of the Material	Residue on a No. 14 lawn	Residue on a No. 16 lawn	Total Residue	Designation of bodies derived from these flints	Designation of glazes derived from these flints
I	Amer. Rock	3.7	8.2	11.90	1 A	1 B
II	" "	0.2	0.3	0.50	2 A	2 B
III	French	4.9	5.9	10.80	3 A	3 B
IV	Sand	1.7	1.5	4.20	4 A	4 B
V	"	0.30	7.3	7.60	5 A	5 B

I first made cones of these five and fired to cone 9. Number 1 had become quite firm, would not mark; number 2, number 3, and number 4 marked easily; number 5 would hardly hold together. I now fired other cones of these same flints to cone 25. Results showed 1, 3 and 5 quite hard; 2 and 4 still soft.

My next step, was to make five bodies, with a fixed formula, using each of the above flints in turn. These I called 1 A, 2 A, etc. Of these, cones were made and fired to cone 9 and cone 25. No marked difference could be noted in the bodies, at either heat. All vitrified at cone 9, none had fused at cone 25. I then cast pieces of pottery from each of these bodies in the same mold, and fired all in the same sagger.

Five glazes were next made with the samples, using a fixed formula, known to be nearly correct for the body formula used, but one which would in time show craze. These were numbered 1B, 2B, etc. Of these cones were made, and the comparative fusibility determined. Glazes using flints 1, 2, 3 and 4 were very uniform, practically identical, those, which were originally the finest ground, are a shade the softest. Glaze using flint No. 5 is plainly a little harder. These five glazes were tried on uniform pieces of the No. 1 A body, precaution being taken at each step, to give a uniform test.

The glazes came from the kiln free from craze and remained so for two days, then 1B began to craze, this was followed by 3B and 4B and then later by 5B, 2B remaining free. So one Rock flint, both sand flints, and the French

flint craze, while the other Rock flint remained free. The kind of flint certainly seemed of no consequence here.

The pieces made from the various bodies were all dipped in glaze 1B. It was a week before any of these showed craze, then 1, 3 and 4B showed a trace, while 2 and 5 were free. So of the glazes, the first to craze was a Rock flint, but the only one that, in time did not craze, was also a Rock flint. Between sand number 4 and French, there is but little choice, while sand number 5 is a little better than these two. Of the bodies, 1, 3 and 4A, show a very slight craze, of which 3A is a little the worse, not enough, however, to warrant saying positively, that the amorphous French showed a noticeably different co-efficient from the others. No. 2A and 5A are free from craze. Drawing conclusions from the experiments, we find, *First*, if there is any difference between the action of French and the others, it must be very slight. *Second*, that with uniform fineness of grain, just as good results can be obtained, in using any of the flints, whether it be Rock, sand or French, in the glaze and as far as can be judged, from these trials, in the body also. *Third*, and finally, most important of all, is the fineness of grain. Number 1 American Rock, the sample containing originally the coarsest grain, showed most craze, while number 2 American Rock, the finest grained sample, shows the least.

DISCUSSION

Mr. Edward C. Stover: I would like to ask whether the testing was done with an ordinary hand lawn, or whether Mr. Burt had any special apparatus for testing the fineness?

Mr. Burt: Just through a hand lawn.

Mr. Stover: I would like to ask what number of meshes to the inch corresponds to your number sixteen?

Mr. Burt: 160 to the lineal inch, or 160 x 160 per square inch.

The Chair: Not exactly that, but nominally that.

Mr. Stover: You did not take what passed the 140 and was rejected by the 160 between the two?

Mr. Burt: No.

Mr. Stover: You took only what went through both?

Mr. Burt: Yes, I had no opportunity to arrange floating apparatus to float off the finest sizes. I aimed to get results in which I would have sufficient confidence to go ahead and use them in any regular work. The point which I make in my paper is that if you compare a flint which passes a number fourteen lawn completely, with another which leaves large per cent residue, the former is going to be much finer flint, not only in the grains which refuse to pass the lawn, but also in those which do pass it. Its grains will average finer that those of the flint has a coarse residue.

Mr. Stover: A gentleman in Trenton once brought up this same question, and I believe there was a series of experiments made by the Eureka Spar Company where the same spar was ground the ordinary length of time, something like four hours, then double that time, than three and four times the usual period. It was then lawned off and they found the difference in fineness did not compensate for the difference in time of grinding. With double grinding, there was little difference in the fineness of the product, measured by passage through the lawns.

Mr. Burt: Mr. President, there is another point which I do not touch upon in my paper. Some people claim you can grind a flint too fine, but I don't think we will ever run any danger from this source.

The Chair: Dr. Zimmer has a paper along this same general line, and unless someone now desires to discuss Mr. Burt's paper further, it would be well to hear Dr. Zimmer's paper before dropping this subject.

HYDRATED SILICIC ACID IN KAOLIN AND ITS EFFECT ON POTTERY BODIES.

BY

WILLIAM H. ZIMMER, PH. D.

The prototype of all our various clays is the mineral kaolinite, the chemical formula of which, Al_2O_3, 2 SiO_2, H_2O, is generally adopted as the formula of clay substance; in fact, up to date, it is the only one considered in ceramic calculations. Yet there seems to be here, in our own country, quite a number of hydrous silicates of alumina, which, although classified in a commercial way as Kaolins, do not correspond exactly to the formula of kaolinite. We find allusions in the Indiana Geological Survey reports, to a mineral Indianaite, which contains Al_2O_3, 2 SiO_2, 4 H_2O, the latter being combined. Cook, in his reports on New Jersey clays, mentions also the presence of free hydrated silica in quite a number of instances.

I venture to say that these cases are not generally known to those interested in the manufacturing of ceramics, and I must confess that I myself was not posted about these facts, when in 1897 I received for testing samples of some southern clays, where the physical properties and chemical analysis seemingly disagreed. The sample was too small for a thorough investigation, and as my time was pretty well occupied by the regular factory work, I had no chance to take the matter up again, until last summer I received another very pure, white sample from about the same locality.

The clay was rather hard and had to be ground in order to mix well with water. It left hardly any residue on a 130 mesh sieve. The linear contraction from the moist, damp state, fit to be worked in the clay-shop, to perfectly air-dry, was 13.64 per cent.; the shrinkage from air-dry to cone 7 was

15.62 per cent., making a total of 29.16 per cent. and surpassing that of all our well-known pottery clays. The clay burnt glass hard at that temperature, and was non-absorbent. Its color was still a splendid white, and, even when glazed, it showed a better color than the very best English china clays. Plasticity and strength were also remarkably high. Introduced into china-bodies,—true porcelain as well as bone-china,—the result was a lowering of the temperature of fusion, giving translucent bodies at a heat decidedly below the one necessary to cause the same degree of translucency where one of the standard kaolins was used.

Still more interesting was the result of the chemical investigation. The rational analysis showed only a residue of 0.05 per cent. not decomposed by H_2SO_4, and according to all rules and regulations of our general ceramic creed, I should have been entitled to take the sample for the ideal pure kaolin of the formula Al_2O_3, 2 SiO_2, 2 H_2O, corresponding to 39.54 per cent. Al_2O_3, 46.51 per cent. SiO_2, 13.95 H_2O. To my greatest surprise the analysis gave the following result:

SiO_2	57.00
Al_2O_3	24.85
Fe_2O_3	0.25
CaO	0.05
H_2O	17.81
	99.96

It is evident that under the old supposition that I would have to deal with a kaolin of the standard formula Al_2O_3, 2 SiO_2, 2 H_2O, the result of the chemical analysis did not agree with the rational analysis. Finally I came to the conclusion that there must be present free hydrated silicic acid, although I did not know of any similar cases, until Professor Orton told me that he had had some experience on the same line, and kindly encouraged me to go ahead with my investigation. I developed for this kaolin sample the formula:

$$Al_2O_3, 2\,SiO_2, 2\,H_2O + 2\,H_2SiO_3 \text{ or}$$
$$Al_2O_3, 4\,SiO_2, 4\,H_2O.$$

The theoretical composition of such a substance would be

SiO_2	57.83
Al_2O_3	24.84
H_2O	17.33

This composition is almost identically the same as given by the chemical analysis. If we are justified in ascribing to his kaolin such a constitution, the result of the rational analysis is explained in a very satisfactory manner, as naturally free hydrated silicic acid will be easily dissolved by by the repeated treatment with NaOH. It will also explain the behavior of the clay in the kiln and in china bodies, as it is to be expected that in presence of fluxes, free silicic acid will become a reagent more active than flint, as soon as the chemically combined water begins to be expelled.

The question now arose as to whether all such kaolins would show free hydrous silicic acid in certain proportions, corresponding to this new formula, or since the relation between the molecules of the clay substance proper and free hydrous silicic acid would be rather loose, whether this latter would not be more or less washed out during the process of decomposition which undoubtedly originated those kaolins. I succeeded in obtaining small samples of two other clay beds, which gave me the following results:

Sample II was also very white and hard as if indurated by hydrous silicic acid. The powdered substance mixed with water would pass through a 150m. sieve without leaving any residue and the rational analysis indicated only 0.10 per cent. mineral detritus.

The pulverized substance was dried 48 hours over H_2SO_4 in a dessicator to apparently constant weight, but lost in drying oven heated to 120° Cels. 2.78 per cent. water, which loss I would attribute to water combined with the silicic acid. After 4 hours drying, the weight was constant. The chemical analysis gave the following figures:

SiO_2	50.00
Al_2O_3	35.28
Fe_2O_3	0.15
CaO	0.57
Loss by ignition Chem.comb. H_2O	14.28
	100.28

This kaolin contained undoubtedly a rather small percentage of free silicic acid and as the 2.78 per cent. water lost in drying oven at 120 degrees Cels. might have included some moisture retained even during the process of drying over cone H_2SO_4. I tried to make a direct determination of the free silicic acid by treating the substance with a solution of caustic soda. All free silicic acid ought to go into solution, but it is very difficult to properly wash the clay on the filter unless you filter under pressure. Even the repeated settling of the substance and decanting of the supernatant liquid is a very slow process. The silicic acid was precipitated by HCl and treated as usual; but as caustic soda might have attacked to a certain extent the clay substance proper, I determined also the small amount of alumina in solution and figured from the alumina the combined silica that was to be subtracted from the total amount of silicic acid dissolved by caustic soda, in order to determine the real amount of the free acid. The result gave me 6.05 per cent. free silicic acid, a percentage which I consider rather too low, on account of imperfect washing out, but which strengthens the proof of the presence of free hydrous silicic acid although the figure might be not quite exact.

Sample III had again the same physical properties as the previous sample, only it was still harder, flinty I might say, and needed grinding in order to mix with water to a plastic body.

The substance was dried at first in desiccator, and lost afterwards in drying oven at 120 degrees Cels. 9.09 per cent. water. Heated in platinum crucible to red heat it lost 7.56 per cent. more. The complete analysis of the air-dry substance gave the following result:

SiO_2	68.63
Al_2O_3	15.00
Fe_2O_3	0.60
CaO, MgO	trace
H_2O	16.65
	100.88

The substance dried at 120 degrees Cels. contained

SiO_2	75.50
Al_2O_3	16.50
Fe_2O_3	0.66
H_2O	7.56
	100.22

By treatment with NaOH, I extracted 31.41 per cent. SiO_2 and the same time by determining the Al_2O_3, I found that 3.97 per cent. clay substance proper had been decomposed. Consequently I had to subtract 1.84 per cent. SiO_2 from total above. This would leave 29.57 per cent. Silica, which had been present in the free hydrous state. Figuring from the loss of 9.09 H_2O in drying oven, there should have been 30.29 per cent. SiO_2. Both figures are undoubtedly rather too low. The rational analysis of the substance dried at 120° gave me a residue 1.26 per cent., containing 0.91 per cent. Silica, while 0.35 per cent. not volatilized by hydrofluoric acid, consisted mostly of Fe_2O_3.

The presence of free hydrated silicic acid was proven in all three samples beyond any doubt, yet I must say that it was impossible to establish one formula covering all three instances unless we would write the formula:

$$Al_2O_3, 2\,SiO_2, 2\,H_2O + x, H_2SiO_3,$$

where x denotes the varying amount of the free hydrated silicic acid. The variation is considerable, but as there are a good many possibilities of having free hydrated silicic acid removed from the remaining kaolin, such variation is not at all surprising and would even not contradict the theory, that the original mineral in all three cases has been the same. Whatever this mineral has been, I do not venture to say, without a more thorough knowledge of the geological condition of those sections of our country. Perhaps these clays originated by decomposition of volcanic tuffs or other plutonic rock on the bottom of the deep sea, which receded in more recent periods and finally left the claybed on dry land. At any rate the presence of such large amounts of free silicic acid is an almost direct proof against the theory of Collins (see Ries. Transact. A. C. S., vol. II, page 94,) that

kaolinite is derived from feldspar by fluoric vapors, which would have probably increased the percentage of alumina, but would hardly have left any perceptible quantity of hydrous silicic acid.

Those three clay samples with free hydrated silicic acid had, however, properties so characteristic that I went further to define more exactly the influence of free hydrated silicic acid on well known clays.

I selected for this purpose Zettlitz kaolin, the analysis of which is given by Seger, Ges. Schriften, page 182, as follows:

SiO_2	46.87
Al_2O_3	38.56
Fe_2O_3	0.83
CaO, MgO	trace
Alkalies	1.06
Loss by ignition	12.73
	100.05

and Tennessee ball clay from L. Mandle, which, according to Mr. S. Geysbeek's rational analysis, consists of 86.20 per cent. clay substance, 2.70 feldspathic minerals and 11.10 per cent. quartz. Both were treated alike. To one pound dry clay, mixed with water to the consistency of a good slip, I added 16 oz. of the ordinary commercial solution of silicate of sodium; the slip was acidulated with HCl and coagulated at once to particles not unsimilar to those of the natural clay sample with free acid when immersed in water. By repeated washing, sieving and pressing, the surplus of muriatic acid and also the chloride of sodium was removed as completely as possible.

On pressing into plaster-paris moulds of 4 inches diameter, in the form of small round disks, these two clay bodies with free hydrated silicic acid behaved in a manner analogous to the three sample clays. They had been indurated by the hydrous silicic acid, and the little disks made from the prepared Zettlitz kaolin stood five times the weight that was sufficient to break one made out of the pure kaolin without any of the hydrated silicic acid. An addition of flint corresponding to the same amount of SiO_2 would have weakened

the strength. In the same way, an addition of flint would reduce shrinkage. Pure Tennessee ball clay will shrink in drying 4.90 per cent.; this percentage will remain almost constant when carrying only small amounts of flint, but will finally drop to 2.80 per cent. in a body of 25 parts Tennessee ball and 75 per cent. Flint and about 1 per cent. in a body with 87.5 per cent. flint.

The following table contrasts the relative shrinkage of the clays with and without the H_2SiO_3 as well as one of the natural samples:

	Pure Zettlitz	Prepared Zettlitz	Pure Tennessee	Prepared Tennessee	Sample No. 1
Shrinkage in drying	1.56	12.50	4.90	16.67	13.54
Shrinkage in fire (cone 8).	6.25	7.50	6.90	13.28	15.62
Total shrinkage ...	7.81	20.00	11.80	29.95	29.16

At the same time, these prepared clay trials containing free hydrated silicic acid showed, after firing, a decidedly better color; a fact which could not be attributed to a mere addition of a white substance like silica, but which probably we will have to explain by a chemical process, in as much as free amorphous silicic acid is more likely to bind Fe_2O_3 to a more or less colorless silicate even in oxidizing fire than the crystalline flint.

I have here (holding up samples) two china bodies, both consisting of Zettlitz kaolin, spar and silica in exactly the same proportion, only one contained originally part of its silica as free hydrated silicic acid. Both were fired at cone 8 in the very same sagger. The fire was nearly all the time oxydizing, yet there is quite a contrast in the color of both and the greater translucency of the one with better color indicates at the same time that free silicic acid is a more active reagent in the heat than quartz, and has a tendency to lower the vitrification point of china bodies. (The samples were here passed around the audience and exhibited a very marked difference in translucency.)

A subsequent experiment led me to change my opinion in this matter, to a certain extent.

I precipitated silicic acid out of a solution of silicate of soda by muriatic acid, and washed for two days until a test with nitrate of silver indicated that the last traces of acid and chloride had been removed. The residue was allowed to dry without any artificial means, and, tested immediately before being used, it yet contained 43 per cent. H_2O.

The spar applied in the following experiment was from F. H. Silkmann, New York, and contained according to the analysis by Sharpless, Boston:

SiO_2	63.50
Al_2O_3	19.68
Fe_2O_3	0.12
Na_2O	4.88
H_2O	11.27
Loss by ignition	0.80
CaO, MgO	traces
	99.75

The kaolin was the same Zettlitz kaolin, used in previous tests. A body, containing 40 per cent. kaolin, 40 per cent. spar and 20 per cent. silica, corresponding to the following actual weights:

Zettlitz kaolin	40.00	
Spar	40.00	
amorph., precipit. Silicic Acid	85.08	(less 43% moisture = 20.00 SiO_2)

was very short and could hardly be worked, while in the previous experiments and in all instances where the natural clay containing free hydrated silicic acid was used, the bodies were very plastic after being properly ground.

A second body was made according to the same formula, and out of the same materials, except that the amorphous silica was replaced by a dry ground flint containing 99 per cent. SiO_2 and 0.51 per cent. moisture. The actual weights were Zettlitz kaolin 40, spar 40, dry ground flint 20.1 part. Although it was a short body, this mixture was very much easier worked than the mixture with amorphous silica. Both were fired in the very same saggar to a temperature between

cones 7 and 8. Mixture A with amorphous silica was nicely translucent, with a slight greenish tint and a linear contraction of 18.75 per cent. Mixture B was also translucent, but had a rather yellowish cast and a contraction of only 10.42 per cent.

These foregoing somewhat contradictory tests seem to indicate that the way in which the amorphous silica is imparted to the mixture has a distinct influence on its behavior in burning. The increased fusibility in one artificial test is not well supported by the second trial, but the behavior of the original clay which contains the free H_2SiO_3 can still be explained only on this hypothesis.

The lowest melting Seger Cone without Fe_2O_3 is cone No. 4, having the formula:

$$\left.\begin{matrix}0.3\ K_2O\\0.7\ CaO\end{matrix}\right\}0.5\ Al_2O_3,\ 4\ SiO_2$$

and in Seger's Gesammelte Schriften, page 191, there are given the following proportions for cone No. 4 mixture.

Rörstrand Spar	83.55 parts
Marble	35.00 "
Norway Quartz (Flint)	54.00 "
Zettlitz Kaolin	25.90 "

Seger treats these materials according to his analytical results as pure, and makes no correction for impurities.

A cone made according to these figures, corrected by the analytical results from the natural clay containing free hydrated silica acid melted exactly with cone No. 3.

Following the same principle, I made two mixtures of cone No. 4, in which I used in one the precipitated air dryed silicic acid, and in the other, ordinary dry ground flint.

MIXTURE A:		MIXTURE B:	
Spar	83.55	Spar	83.55
Whiting	35.00	Whiting	35.00
Amorph. SiO_2 cont. 43 per cent. H_2O	94.74	Dry ground flint (0.51 per cent. moisture)	54.27
Zettlitz kaolin	25.90	Zettlitz kaolin	25.90

Both were tested repeatedly, but I have to state that I could not discover any difference in the final melting point.

The shrinkage of Mixture A was again higher than that of Mixture B. Yet we must recollect that the naturally occurring clay containing free silicic acid depressed the melting point one whole cone number or about 20° C., when used in place of Zettlitz kaolin.

So far as I know, all ceramists who have tried to give a rule about fusibility have treated of silica only in its crystalline form. So does Bishop in his refractory quotient, and even Seger has based his theory only on the ratio of the fluxing constituents to the alumina and to silica. Of course, there is no clay containing free hydrated silicia acid among the German clays generally known up-to-date, but there is no doubt that not only the ratio of fluxing constituents to alumina and to silica, but also the state and condition of silica, whether crystalline or amorphous, has a far-reaching influence on the fusibility. I believe that we have in our country in those clays with free hydrated silicic acid, the means to manufacture porcelain at temperatures far below the heat applied in Europe, and I would not be surprised if the reason why Seger only partly succeeded in manufacturing porcelain at the same low temperature as the Japanese, would be found in the fact that Japanese use clay with free amorphous silica, while Seger had always to deal with silica in its crystalline form. Of course this is only hypothesis, as far as the Japanese are concerned.

For our American manufacturer, the lowering of the temperature in burning porcelain will prove to be of undoubted value in the future, as such a reduction of heat does not only mean the saving of fuel, but reduces at the same time the strain on saggars, their loss and last but not least it reduces the wear and tear of our kilns.

It is true that clay substance, freed of every bit of moisture and chemically combined water, is not plastic and that, besides shape and size of the smallest individual clay particles, the chemical composition is of the greatest importance for plasticity, so that we may say, that clays differ in a marked degree in plasticity according to the amount of interstitial water, of which there is an unusually large amount contained in clays with free hydrated silicic acid;

yet this very fact, which assists in reaching a very desirable degree of plasticity, will also prove a source of danger to the manufacturer.

It will and must increase shrinkage. A mass of clay may be looked upon as a labyrinth of pores, of capillary tubes, and while shrinkage is not only a phenomenon denoting a removing of water, but also indicates a consolidation, bringing the clay molecules nearer to each other, that is within the realm of more active attraction and action, still the potter has to be afraid of any excessive contraction, which may alter his shapes or cause his ware to warp in drying.

Crooked ware however is not the only danger. We know that water, especially when containing traces of alkalies, will dissolve hydrated silicic acid. In drying, the capillary tubes will carry this solution to the surface where the water will evaporate, while the remaining silicic acid will form a solid film, which will finally prevent an even drying, just as it will prevent the natural dry clay from "chalking" in water and sometimes will make it appear gritty and unplastic until the particles are torn apart by being forced through a sieve. The surface of a clay body with hydrated silicic acid will be dry already when the interior is still moist and the surface-tension caused thereby may become so great, that the ware will crack to pieces. This is especially dangerous in larger pieces of ware. At the first glance in making thin goods, the exceedingly quick hardening of the surface, the quick "letting loose" from the moulds, seems to be only an advantage, in the fact that the plaster paris moulds can be emptied quicker, and can be used more frequently.

The foregoing studies on these natural clays, and with synthetic mixtures designed to imitate them, have led us up to the following conclusions:

That the presence of any important quantity of free hydrated silicic acid in a clay tends:

First, to produce an increase of translucency over that obtained where the silica used is all quartzitic, at equal temperatures.

Second, to bring about an improvement in color.

Third, to increase their shrinkage both in air and in fire.

Fourth, to produce a lowering of the temperature at which vitrification occurs. This point was supported by tests of the natural clay, but only partly supported by synthetic mixtures.

Fifth, a tendency to warp in drying.

Sixth, a tendency to form a hard coating on the surface, due to the deposition of H_2SiO_3 from water used in making wares plastic.

If these few disadvantages can be regulated or overcome, it seems very likely that the American potter will find opening before him a perfectly new field, and that the clays containing free silicic acid will be very likely to become a very important factor in the development of true American hard spar-porcelain.

DISCUSSION.

Mr. Ernest Mayer: Dr. Zimmer has given us a very valuable paper, and I don't suppose there is a person in the room who can follow such an array of figures and be ready to discuss the paper at once.

The suggestion has been made that we discuss Mr. Burt's and Dr. Zimmer's papers together. I shall not attempt to discuss Dr. Zimmer's, for I will admit I do not thoroughly comprehend it yet. I want to see it in print before attempting to do so, as it will require some study. Mr. Burt's paper is right in line with the idea that I have been advancing ever since I have belonged to the American Ceramic Society and long before the Society was ever thought of.

When you speak of the *fine grinding of materials* I consider you are in the very heart of the greatest economic problem of the white ware business, because it has one of the greatest, if not the greatest, bearing on the question of reliable ware.

We all admit, I presume, that we mix our bodies and glazes in some kind of a systematic manner. In a body, for

instance, a certain per cent. of this Feldspar, Ball and China clays are mixed together with a definite view in our minds as to what part each ingredient shall play.

Suppose a man making a body will leave it in his tanks over night and in the morning starts pumping out of the tanks right into the presses. It is impossible to tell what he is getting there; certainly it is not the body originally made, because the larger particles have settled down to the bottom. The English manufacturer takes his flint and stone in the liquid form. What would become of ours if we did that? We could not start our mills, if once they stopped. We could hardly dig them out. In England the flint and cornish stone is so finely ground, they can carry it for miles, and run it out into barrels, through small spigots. We could not do anything of that kind. Take a handful of our flint, mix it with water and while you are talking about it, it has settled down to the bottom in a hard mass. Take the English flint and mix it in water and it will stay there in suspension for a considerable length of time.

The Chair: Do you think that it is equally the case with French flint and rock flint?

Mr. Mayer: I don't think it makes any difference what kind of flint is used. Of course, all the flint they have in English potteries is from the Yorkshire coast — they don't know anything about rock flint. But if you will grind ours fine, it will act the same way theirs does. It simply means this: as soon as we grind our flint down properly, we will get the same results they do. But you can't get a flint maker in the United States to grind that way.

Mr. Edward C. Stover: How do they grind their flint in England?

Mr. Mayer: It is always wet ground.

Mr. Stover: The experiments I referred to in reference to grinding spar were made on a dry-grinding ball mill. Thomas Maddock & Sons explained the difficulty in getting a fine grind as a result of the use of a ball mill.

Mr. Burt: They can certainly grind as fine as they want to in a ball mill, if they grind wet.

Professor Edward Orton, Jr.: Mr. Mayer has made the statement, that he thinks the fine grinding of English flint makes it float in the water better?

Mr. Mayer: I do not know of any other explanation to offer. In England, if they go to a certain point in grinding it is all right, but if they stop too soon, it settles so hard they can't dig it out. In our materials, if we will grind and grind till very fine, we will have no trouble at all about this question of settling. But if it is coarse, it will settle, and we can't stop it.

Professor Orton: I wanted to state in regard to that point, the fact that Mr. Mayer has just brought out in his last remark, viz.: that between certain limits, that settling action takes place. I never made any experiments in regard to the fine grinding of flint, but I did, at one time, grind some window-glass down very fine. On one occasion I allowed it to set quiet awhile, and I had to take a chisel to get it up off from the bottom of the vessel containing it. It had woven itself into almost a cement on the bottom. I also found that the top of this cake consisted of the finest material which had remained longest in suspension, and it was not as hard to loosen up as the coarser material which underlay it. There was a clear line of demarkation between the two sediments. The line could be pretty readily seen between those particles which had settled readily, and those which settled more slowly. We could peel this top layer of fine material off in a sort of leathery film of a semi-plastic nature; that in the bottom, we had to dig out. It is also true, that if materials are coarse ground, we get from them no tenacity. It is only between certain limits of size of grain that this peculiar tendency to settle in such a hard cake takes place. Just what those limits are, I do not know, but I think the problem is one which we could easily solve by experiment.

Mr. Stover: In the preparatory work for your paper last year, the material was fine ground?

Professor Orton: No; not very. All of the sizes of material I was working with, at that time, would settle if

left quiet for an hour or so. Compared to the material Mr. Mayer has described, my sands of last year were all coarse.

John W. Hasburg: I have not investigated the different varieties of flint or silica referred to by Mr. Burt by tests or comparisons thoroughly enough to make specific statements, but will say there is a great difference according to the source from which the silica is derived, whether it is mineral, organic, or chemical formation; also, if it is of a hydrous or anhydrous nature.

By comparing an opal or amorphous silica with rock crystal the physical difference is that the amorphous silica is a porous spongy material, while the rock crystal is a solid compact mass not at all porous.

Rock crystal, however fine it may be ground, will always be sharp solid crystals, while the amorphous variety will always be porous.

Caking or setting in the mill may be caused by hydrated silica. Some materials have the property of "setting" when mixed with a certain amount of water, but will not do so if an excess of water is used. If such a material is ground wet after having been mixed with water previously it will not set again.

Now if setting should be caused by hydrated silica it could easily be remedied by calcining. This will decompose the H_2SiO_8 to SiO_2 which will no longer be affected by water. Calcining does not destroy the porosity of the amorphous silica.

The "bond" of amorphous silicas is stronger than that of the rock crystal, also the amorphous silica is apt to fuse at a lower heat than rock crystal on account of the spongy structure which permits the other materials to work into the pores and effect combination more easily.

May this not be a probable explanation why in Europe they can fire at a lower heat than here?

Prof. Edward Orton, Jr.: There have been one or two points in my experience that would partly bear out Dr. Zimmer's work; also, some points in his work which throw some light on what I have not heretofore understood. Regarding this Indiana mineral, (I think Dr. Zimmer men-

tioned it) which is supposed to have the usual proportions of Al_2O_3 and SiO_2 and four molecules of H_2O, I would say that it has the peculiar conchoidal fracture which the flint fire clays all show. Why should both clays have this peculiarity in common?

And, two or three years ago, I received from Professor Dumble, formerly State Geologist of Texas, some material concerning which he wanted a report. We analyzed it, and found, as nearly as I can recollect, about 65 per cent. of silica, but it seemed excessively fine grained; when we burnt it in the kiln, we found that the raw material as delivered became glass-hard at cone 02. I have actually taken a piece of it and cut window glass nicely, after coming from a terra cotta kiln, fired to cone 02. Nothing had occurred in my previous experience to explain a case of that sort. It was not strongly discolored by iron, only to a pale yellow. There was no evidence whatever of lime; I tested for it by hydro-chloric acid, and got nothing. We then thought of hydrated silicic acid as the cause, and while my laboratory assistant was working on this theory, I wrote to Professor Dumble and asked the probable origin of the clay. He stated that the deposit was a peculiar one, extending in depth five or six feet over a considerable area of country; he viewed it as the decomposed remains of a volcanic ash deposit. He said he believed the country there had been pretty well covered over by a layer of very fine ash from volcanic eruptions in the mountain country west of there. There had evidently been a period of volcanic activity in Western Texas, but not in the present geological epoch.

The ashes ejected from volcanoes are merely fine particles of lava, not ashes in the usual sense. A great many volcanoes blow out great quantities of this lava-dust. Professor Whymper mentions a volcanic eruption in the Andes, S. A., where a column of dust thirty thousand feet high was sent up before it reached a current of air strong enough to overcome its vertical motion. Then, a strong wind broke the column off at right angles and carried it off across the country, falling in a fine shower of dust. So we see that volcanic dust may, under certain conditions, travel long distances in

this excessively fine state. Now this dust is nothing more nor less than the same kind of rock which is at other times ejected from volcanoes as lava. That is, it is a silicate rock. If blown out as dust, it is certain to be chilled quickly; if it flows in great lava streams, it will probably cool slowly and crystalize into a coarse-grained rock. If it chills quickly, it will be exceptionally susceptible of attack by the processes by which kaolin is formed. So we see that such dust, deposited in a thin layer over a large area of ground, and subsequently weathering there, would tend to form a kaolin bed.

But, in the process of kaolinization of feldspathic silicates, we have never yet successfully accounted for the great quantity of silica which is set free, and of which kaolins ordinarily show no trace. For instance, feldspar may be considered to be

$$K_2O,\ Al_2O_3,\ 6\,SiO_2$$

In kaolinizing, we have assumed that the K_2O is converted to K_2CO_3 and taken up by water or by plants. Also, we have assumed that the Al_2O_3 unites with molecules of the silica and water to form

$$Al_2O_3,\ 2\,SiO_2,\ H_2O$$

But, four of the six molecules of SiO_2 originally in the spar are still unaccounted for. They may be taken up by plants; we know that some species, notably the bamboo, do store up quite a quantity in their tissues. Or the silica may crystalize again into quartz. This does not seem likely, because if it did, we ought to find some of the quartz crystals on the kaolin. We often do find plenty of quartz there, but generally as sand grains, which have obviously not crystalized where they are found, but have been formed by the grinding and working down of other quartz crystals of earlier formation. Or, water may dissolve it. But we know that only water containing alkaline salts dissolves silicic acid freely.

So, I repeat, that our explanation of the formation of kaolin has always been very lame. Now, when we find a clay, derived apparently from volcanic ash of comparatively

recent geological age, and find it still lying *in situ*, just where it fell from the sky, and in a comparatively arid, poorly watered and poorly drained country, and find furthermore, that it contains a large amount of free hydrated silicic acid, I do not think we have occasion to feel any great surprise. It looks merely as if the SiO_2 has not had time or opportunity to disappear in that manner, whatever it may be, which it usually takes. It seems to me more surprising that this class of clays are not found oftener, instead of so seldom. In fact, I think many such would be found in our ordinary clays, if we were in the habit of looking for them.

I have enjoyed exceedingly Dr. Zimmer's presentation of this case, and it seems he has shown most ingeniously the effect of hydrated silicic acid upon the clay with which he has found it associated. This suggestion is also valuable to me, because I believe along this general line, we will find an explanation of the so-called flint clays, which we generally speak of as non-plastic. They become plastic, if at all, only upon excessively fine and long-continued grinding. We find them in the carboniferous strata in Ohio and in some other states in strata of other geological age. They are often practically pure kaolinites in composition, but also practically devoid of plasticity; the very antithesis of plasticity, in fact. They are so sharp, that they can cut the skin. The miners use gloves, to avoid being cut from flying chips of the clay.

I have examined out-crops of flint clays, where they lie near the surface, and have been weathering for no one knows how many centuries, and even where they have been ground by passing wagon-wheels to a fine, gritty sand; but even under these conditions they still show no signs of plasticity. We also find similar clays, with the same peculiar flinty structure, but containing five or ten per cent. of silica over and above what is necessary to form kaolinite with the alumina present.

We find these flint-clays interstratified with other fire-clays of the plastic variety; some times alternating in the same bed. In Ohio, they are invariably the under-clays of a coal formation, but I am told that in other states they are

found with no coal or evidence of coal-forming conditions having been present when the clays were laid down.

Why should one clay be hard as flint and practically non-plastic, while another, formed within a foot of it, and not differing sensibly in its gross chemical composition, is plastic? This problem has long been vexing students of the clay industry and has yet to be solved.

But Dr. Zimmer speaks of this clay of his having a peculiar, hard, smooth surface and not becoming plastic until very finely ground. Other clays of the sort which I have seen, have this peculiar conchoidal fracture and inability to mix with water also, which characterizes the flint clay. It seems to me possible that many of our flint clays may be found to differ from other clays principally in the way their silica is present, rather than in its amount. There still remain some inconsistencies to explain, but, after all, there is a fruitful lead, it seems to me, in this idea of hydrated silicic acid and the development of the flint or non-plastic structure in clays. It is a fine field for one of our younger members to take up and explore in the light of the interesting developments which Dr. Zimmer has brought forward.

Another reason for thinking that hydrated silicic acid is formed under the conditions prevailing during the growth of the coal measures, in which these clays most plentifully occur, is that swamp conditions now act vigorously to decompose minerals which, in streams and running water, are not attacked. Swamps beget organic acids and mineral acids also by the decomposition of the organic matter which lies rotting on their slimy bottoms, and these acids are unknown factors in the decay and alteration of the rocks with which they come in contact. Sand-stone, exposed to swamp action for long periods, has been found superficially converted into amorphous quartzite by the agencies which it there met. If this is so, why should not clays also show the occasional conversion of their sands into hydrated silicic acid, and under the agency of this substance assume the hard, flinty and reluctantly plastic condition in which we find them?

I think Dr. Zimmer's paper is a strong addition to our literature, and congratulate him upon it.

Mr. Burt: Dr. Zimmer calls attention to the increased fusibility of his bodies from the fact of the silica being present in the amorphous state. It has occurred to me that perhaps that might have some connection with the composition of spar. It has always seemed peculiar that spar, with its high percentage of flux and low percentage or silica, should have nearly the same fusing point as Cornwall stone. Do you think the silica there might also be in the amorphous state?

Dr. Zimmer: I hardly think so. I did not investigate on that line. This body which I made almost on the line of natural Cornwall stone; about fifty per cent of spar, twenty-five per cent. of flint and twenty-five per cent. kaolin, or something like it. That mixture, melted, had the same appearance at cone number seven or eight as the natural Cornwall stone. It was almost identically the same.

Mr. William D. Gates: I want to say right here that I think this paper is one of the treasures of the Association, and if the Association passed away and had only produced one paper like this, it would have had sufficient excuse for existence. I shall await its appearance in print with the greatest interest.

The Chair: I would like to say before we close this discussion, how much I have enjoyed both papers. There are one or two points which I would like to bring before you. First, is the size of the grain, which I think Mr. Burt has demonstrated has a great influence upon the behavior of both body and glaze. I do not know that it is a point which any of us doubt very much; but still it is a very great satisfaction to have the thing properly demonstrated. The question of the behavior of the fine grained material, as Mr. Mayer has raised it, is also of the highest importance to all of us. Some regard the settling or not settling as evidence of fine grinding, and I regard it as such. The two papers hang together in this direction. The hydrated silicic acid is of course capable of almost perfect distribution in a body and after dehydration by the fire, plays the part of a finely

ground material—in the potter's words, "it goes farther." I have examined some of these clays, and have been much surprised at their behavior, which I could not understand. It has now been made clear to me. The clays in North Carolina are granular, something like well-decomposed Cornwall stone, but become punky on grinding, and not plastic. These hard gritty clays, as has been shown, are similar to kaolinite in composition, but not in behavior. One becomes plastic and one does not. One point to which I want to allude is that made by Mr. Collins, whom Dr. Ries quoted last year, as to the production of kaolin by the action of hydrofluoric vapors. I think it was pointed out that while this was true of Cornwall clays, it was not true in America. This may show why we do not find hydrated silicic acid in Cornwall clays, and why we do find it in clays in this country—the fluoric action may account for this.

A METHOD FOR THE RATIONAL ANALYSIS OF CLAYS FOR CEMENT MANUFACTURE.*

BY

ALBERT V. BLEININGER, B. Sc.

The valuation of cement making materials is most generally based upon the results of chemical gross analysis. In this respect there is considered the fact that the alumina contents of the finished product must lie between the limits of 4.2 and 9.5 per cent., that of silica between 19.8 and 26.5 per cent., that of lime between 57 and 65 per cent., and that the magnesia contents should not exceed 3 per cent. Outside of these chemical limits there is a more or less well defined appreciation of the physical character of the materials, in regard to hardness, toughness, coarseness of grain, etc., but in the specific determination of the fitness of raw materials, some of the most important chemical and physical considerations are most frequently neglected. Since for the cement making process, the form of the silica and alumina which are to unite with the lime to form silicates and aluminates is of great importance in respect to the ease of perfecting the chemical union, the materials which furnish these constituents, the clays, should be put through tests determining their mineralogical and physical character. Thus a clay containing a large percentage of coarse silica in the shape of quartz grains, offers greater difficulties in the way of producing an intimate mixture, than one having the silica in the form of a silicate or a very fine dust.

Not only are the difficulties of manufacture to be considered, but also the fact that the quality of cement made from coarse clays is always apt to be inferior, owing to imperfect chemical combination, and in order to overcome this

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the capacity of expensive machinery must be reduced, involving extra wear and tear and an attendant increase in cost.

It is the object of this paper to summarize the methods hitherto employed for bringing out the physical and chemical characteristics of clays, and to offer a method intended to accomplish this in a practically feasible way, to the practical execution of which the Ohio Geological Survey has given its attention during the summer of 1900.

The problem of determining the physical and mineralogical character of clays has been attacked both from the strictly physical and the chemical side.

In briefly reviewing the physical methods we find two processes in the field, representing two methods of sizing particles, those of elutriation and sedimentation. The first, represented chiefly by the Schoene apparatus, is based on the fact that the carrying power of a current of water is a function of its velocity, and that hence a stream of water can carry larger particles, the greater its velocity. Thus, by varying the velocity from the lowest to the highest one adopted, a series of sizes of particles is obtained, and the clay being arbitrarily separated according to the sizes of its constituent grains. The Schoene apparatus, though able to give good results, is tedious in operation and not well suited for commercial work.

The second kind of mechanical analysis based on the principle of sedimentation is perhaps best represented by the Whitney process. This depends upon the fact that larger and therefore heavier particles, assuming a uniform specific gravity, when suspended in water, sink more rapidly than smaller ones. The grains, making up the clays, are in this way separated into any convenient number of sizes, usually about six, corresponding to different periods of suspension, checked by microscopic measurement. This method permits of accurate work when properly carried out, but it also suffers under the disadvantage of slowness of operation, and the importance of the personal factor in manipulation.

In regard to the value of these mechanical analyses there may be said, that though they furnish valuable data supplementary to the chemical analysis, yet alone they are lacking in a very important respect, since they simply separate the grains according to size, but do not give any information whatever in regard to their mineralogical character. Thus it is essential to know whether a certain size of grain is composed of particles of quartz or feldspar, which, owing to their chemical character, behave quite differently in contact with lime at a high temperature.

Of the chemical methods there are to be mentioned the methods of rational analyses as devised by Prof. Seger and its modifications by Prof. Fresenius (Quant. Analysis, Vol. II, page 352,) and Dr. Heintzel, and a method suggested by Prof. Lunge and Dr. Schochor. These will now be taken up in the order named.

In brief, Prof. Seger makes use of the fact that boiling concentrated sulphuric acid dissolves that portion of a clay corresponding more or less to the so-called clay substance, but does not to any extent attack feldspar and quartz, the other two essential constituents of clay. Hence by treating with smoking sulphuric acid and following this by a treatment with hot carbonate of soda for the removal of the gelatinous silica, it is possible to remove the clay substance (approximately Al_2O_3, 2 SiO_2, 2 H_2O) from the mineral detritus, feldspar, quartz and micaceous minerals. From the analysis of this residue, the per cent. of feldspathic minerals and quartz can be calculated, using the ratio of silica to alumina in feldspar as a basis.

This method, though giving extremely valuable data in regard to the mineralogical character of a clay, yet neglects to give expression to the coarseness or fineness of the grains composing the matter insoluble in acid, and hence fails to bring out the degree to which the clay unites with the lime to form the silicates and aluminates of lime. In order to overcome this to a certain extent, a mechanical analysis would have to be made of the insoluble residue, which, however, would still fail to tell just which of the various sizes are feldspar and which quartz. Lime can decompose

and unite with the constituents of feldspar, the latter being a silicate, with far greater ease than with quartz, both being assumed as belonging to the same size of grain. Hence we see that the ordinary rational analysis is deficient and does not produce exactly the kind of data desired.

The Fresenius and Heintzel methods are especially intended to show the fitness of calcareous clays, that is, marls, for cement purposes and assume that the solubility of the raw material in acid is a criterion of its value. This opinion, though accepted by a number of chemists, is erroneous, as has been amply shown by Prof. Lunge and Dr. Schochor.

Lunge and Schochor proceed on an entirely different basis and make use of the fact, more akin to the purpose in view, that silicates are unlocked by being heated in content with CaO, as is clearly shown by the J. Lawrence Smith method of alkali determination. These investigators, though they did not work with clays proper, but with marls, succeeded in showing that the CaO attacks most readily the combined silica and the silica present in a fine state of division, thus giving a direct expression of the cement-making value of the materials. By heating sufficiently, treating with hydrochloric acid and sodium carbonate the unlocked bases and the soluble silicic acid are removed, and the residue remaining is simply the matter which was not rendered soluble by the action of the CaO during the heating process.

The work by Messrs. Lunge and Schochor in regard to this investigation was very extensive, and, before adopting their final method they made very thorough preliminary tests. The results obtained by them may be summed up as follows:

1. The solubility of raw marls in acid is not a sufficient criterion for determining their hydraulic value because acids are not proper regents for determining unlocked silica, and since silicic acid itself is soluble in acid only in proportion to the amount of chemical water contained by it. The acid treatment does not discriminate between coarse and fine

silica, between amorphous silica and crystaline silica, nor between feldspar and silica.

2. However amorphous silica and quartz may differ otherwise, they show but little variation in behavior in burning with lime. The fineness of grinding is far more important. Although with the same degree of fineness and the same mode of mixing with equal amounts of CaO and heating under similar conditions, amorphous silica gives rise to a higher percentage of soluble silica than the quartz or a mixture of the two kinds of acid, yet when ground sufficiently fine, even pure quartz is acted upon almost completely and much more so than amorphous silica not so fine, but still extremely fine from the ordinary standpoint. Since the activity of silicic acid depends upon its contents of chemical water, after ignition, it does not differ from so much quartz.

3. Chemically combined silica is almost completely unlocked. No matter how fine free silica is, it is always attacked less than silica in combination and hence it is shown that the best source of silica for cement purposes is a silicate, as far as transportation of the silica to the line is concerned.

Lunge and Schochor have thus indicated a way by means of which it is possible to determine the per cent. of chemically active material in a clay, to separate the combined and very fine silica from the coarser quartz, permitting the statement that the higher the per cent. of silicate and very fine quartz is in a clay the higher is the value of the latter for hydraulic purposes. In other words, the higher the per cent. of dissolved material after heating with CaO and treating with HCl and Na_2CO_3 the higher is the value of the clay for hydraulic cements.

The method of working adopted by the two investigators named was as follows:

Samples of marl were taken, in the shape of lumps of walnut size which were heated in a gas furnace to a temperature of about 1100° C. without pulverizing or mixing, for two hours. These burned samples were pulverized, treated with HCl and Na_2CO_3 the residue being weighed after thor-

ough washing. The value of the respective marls was expressed inversely by the per cent. of residue.

In order to prove that these tests really stand for expressions of the hydraulic value of the marls, Lunge and Schochor proceeded to grind the materials and to make cements of them by burning to vitrification and grinding the resulting clinkers. The burning was done in a small test kiln. The hydraulic cements thus produced were subjected to the usual cement tests, which proved strikingly that for the marls examined, the hydraulic value was a function of the solubility of the clayey constituents when treated as mentioned above.

Upon studying the results obtained by Lunge and Schochor, Prof. Edward Orton, jr., suggested the use of this method for the testing of clays in regard to their suitability for cement manufacture. The investigators just named restricted their method to marls, and although it is extremely valuable for the determination of the hydraulic value of any one marl, yet its results are not generally applicable and must always be restricted to this class of material. The factors in this method are evidently the variables of the clay substance, coarse and fine quartz, coarse and fine feldspathic matter, and in addition to this the lime contents. This evidently may in some cases give rise to conflicting results, as for instance, when there are two marls to be examined of which one has a coarser grain of sand but a higher contents of lime than the second marl, which is fine grained and has a higher percentage of combined silica but a lower per cent. of lime. The method now suggested for clays adopts a constant amount of lime, thus eliminating one variable.

The identity of structure which Lunge and Schohor are so careful to preserve by burning the materials in lumps is not interfered with by reducing them, *without grinding,* to the size of a 40 mesh sieve. In fact by burning the materials in lumps the outside factor of irregularity due to natural formation is so great as to introduce an additional source of error. For a product as homogeneous as cement the consideration of the bed structure is entirely unnecessary.

Thus the process of the Ohio Geological Survey is intended to determine the hydraulic factor of a clay by reducing the latter without grinding, with the assistance of water, so that it will pass through a 40 mesh sieve, care being taken not to reject the coarser material left on the sieve nor to break up any particles by grinding. When in the state of a slurry, lime is added in the shape of precipitated calcium carbonate and thoroughly stirred with the clay. This mass is now dried down and rubbed through a 40 mesh sieve, mixed thoroughly on a sheet of glazed paper by means of spatula and burned in a furnace. A weighed amount of the ignited mixture is treated with hot hydrochloric acid for five hours, filtered and washed, and the residue is treated with a 10 per cent. solution of sodium carbonate under heating, until no more flocculent silica is observed on testing with ammonium chloride.

The Lunge and Shochor method and that of the Ohio Geological Survey differ in the following respects:

1. The above named investigators restrict their method to marls, while the Survey extends it to the testing of all clays.

2. The former work with the natural materials, having varying percentages of lime, while the latter maintains a constant amount of lime by adding the lime lacking, in the form of pure calcium carbonate and also having the lime present in excess.

3. Lunge and Schochor ignite the materials in the shape of lumps taken from the deposit; the Survey reduces them so that the soft portion will pass through a 40 mesh sieve with the aid of water and accomplishing the mixture of clay and lime when the former is in the condition of a slurry.

Having thus outlined the method of working, there now remains to prove that it really gives expression to a co-efficient which is consistent with actual results in the manufacture of cement. For this purpose it was decided to bring the method as nearly possible to uniform conditions of operation and to agree upon a method of mixing, burning and analysis, and finally to make cements out of the materials

thus analyzed, which are to be tested according to the usual specifications for Portland cements.

The description of the work done naturally falls under the following headings:

1. Sampling and preparation.
2. Mixing.
3. Burning.

SAMPLING AND PREPARATION.

The clays arrive at the laboratory of the Ohio State University Ceramic Department in sacks, in the shape in which they were sent by the field collector. These clays, according to their nature, must be treated in different ways. Clays of the type of soft, alluvial clays, after being put through an 8-mesh sieve and repeatedly quartered according to the quantity on hand, are dried at 100 degrees C. and about 500 g, is reserved for the analysis, being run, as far as possible, through a 40-mesh sieve and thoroughly mixed; the coarser portion is, of course, added to the finer. If the material is a hard shale, it is necessary to break it up to the size corresponding to an 8-mesh sieve. A quantity of the reduced shale is now heated at 100 degrees C. till perfectly dry and then allowed to soak in water for 24 hours. If this is not sufficient, boiling is resorted to, which is kept up for 6 to 8 hours, if necessary. The mass is now evaporated to dryness and rubbed through a 40-mesh sieve. For very hard material, rubbing with a rubber pestle may be permitted.

MIXING.

The sample is now ready to be mixed with the lime. This is done by weighing out, say 25 grains of the clay to be examined, whose gross analysis must be known, stirring it up with water to a thin slip and adding reprecipitated calcium carbonate. This mixture is to be stirred thoroughly, evaporated to dryness, rubbed through a 20-mesh sieve and thoroughly mixed with a spatula.

At this stage of the work it was necessary to determine the proper amount of lime to be added to a clay so as to

attain the object sought, that is, the decomposition of the clay substance proper and the formation of a lime silicate, without running the danger of fusing the resulting mixture at the temperature employed in heating. In order to accomplish this a series of mixtures was made, employing a very fine-grained, soft clay, with little bonding power which is used in the manufacture of Portland cement, at Harper, Ohio. The clay has the following analysis:

SiO_2	50.70
$Al_2O_3+Fe_2O_3$	17.00
$CaCO_3$	16.80
$MgCO_3$	9.80

The clay by itself was made the start of the series and the lime was increased as shown by the following table. The mixtures were burned at a temperature of 1100 degrees C, this heat being maintained for 75 minutes.

No.	Per Cent. of Lime Carbonate	Per Cent. Residue	Remarks
1	16.8	....	Clay alone.
2	25.	....	
3	30.	....	
4	35.	....	More or less completely fused.
5	40.	....	
6	45.	....	
7	50.	....	
8	55.	....	Vitrified.
9	60.	3.55	Quite hard.
10	65.	....	Hard.
11	75.	1.12	Soft, but somewhat too hard for easy reduction.
12	80.	2.16	Soft.
13	85.	3.00	Soft and friable.

The mixtures of clay and calcium carbonate were made up into briquets weighing about 75 grams, with the aid of water, placed upon a biscuit plate and burned in the small furnace used for this purpose. No. 11 of the table must be considered doubtful, an error having certainly been made in the analytical work. The results of the table show that it is essential to have at least 75 per cent. of calcium carbonate present in the mixture, not so much because less lime might not unlock the silicate or not attack the free silica, but owing to the fact that the resulting mass after burning

becomes hard and stone-like and hence inconvenient for reduction. For this reason it was decided to use a standard mixture of 80 per cent. $CaCO_3$ and 20 per cent. of the clay to be tested, which results in a friable mass easily reduced. This amount of lime also allows for a sufficient excess, so that uniformity is obtained as far as the reagent is concerned.

In the determination of the insoluble residue of these mixtures sufficient material was taken to correspond to two grams of the clay. In this and other series the total lime was determined volumetrically in a number of analyses. This was done in order to express the per cent. of residue in terms of the actual weight of clay taken, thus making due allowance for the lime introduced as reagent, since the mixing of clay and lime can never be so intimate as to make it permissible to weigh out any amount of the mixture and obtain the actual amount of clay taken by calculation, depending upon the per cent. of calcium carbonate originally added under the supposition that the mixture is perfect. Later on this method was modified in such a way that a 2 grams of the dried clay were weighed out, transferred to a beaker and made up into slip with water. To this was added the amount of lime corresponding to the required percentage. The mixture was carefully transferred to a porcelain crucible, evaporated to dryness, mixed on a sheet of glazed paper with the spatula and returned to the crucible in which the sample was burned, being covered with a lid. In this way, of course, the determination of lime becomes unnecessary.

The analysis proper was carried out as already indicated. Two grams of sample were taken and treated with 40 c. c. hydrochloric acid (1:3), allowing to boil for five minutes till in solution. The solution is now filtered and washed till nearly free from chlorine, the residue being washed back into the beaker and treated with sodium carbonate solution (8 grams per 100 c. c.), 100 c. c. being used each time. The treatment is continued till no more precipitation occurs with NH_4Cl. The residue is then treated with hydrochloric acid (1:4), washed free from chlorine, ignited and weighed.

The main precaution to be observed in this work is to work as hot as possible, but yet allowing the silicic acid to settle. The use of long stemmed funnels is to be preferred to the suction flasks.

The analytical work was done by Mr. Ralph W. Nauss, assistant on the Ohio Geological Survey.

BURNING.

For burning the mixture, there was available a dental furnace manufactured by the American Gas Furnace Co. which was intended to be fired with gasoline vapor, but which was modified so as to burn ordinary kerosine. For this purpose a ¼ inch pipe was introduced into the lower cylindrical part of the furnace. The pipe was run to a can fastened to the wall and was fed with oil from the reservoir by means of a stop cock and funnel. The oil dripped into the open furnace making a sight feed arrangement. The blast was furnished by a positive rotary blower through a ¾ inch pipe, operated during the summer by a gasoline engine.

The mode of operating the furnace was as follows: At the bottom of the cylinder beneath the furnace proper there was placed a small shallow iron pan into which the oil dripped from the feed pipe and which served as an evaporator for the oil. When ready for operation a small bunch of waste saturated with oil was placed upon the pan, ignited and covered with a few pieces of wood and charcoal. Upon this small charge of fuel there were now placed pieces of fire brick, the size of a large walnut, which filled up the cylinder completely and served the purpose of offering resistance to the passage of gases and air, thus mixing them for proper combustion. The air was blown in through three small tuyeres distributed around the cylinder. As soon as the charcoal is fully ignited, the kerosene is allowed to flow slowly and a weak blast is maintained. Gradually the flow of oil and the blast are increased, until the maximum supply is reached.

Before the supply of oil has commenced, a fire-clay tile is now placed across the furnace occupying the space formerly taken up by the muffle. Upon this tile are placed

the crucibles or biscuit saucers carrying the samples to be ignited. At the same time a porcelain tube containing the platinum wire and thermo-couple of a Chatelier pyrometer is placed in position, just touching the mixtures to be heated.

The rise in temperature is readily followed by means of the pyrometer whose millivolt meter is set up in a closet at a distance of about 7 feet from the furnace.

The question now arising is in regard to the temperature to be employed in the burning. It is at once evident that the work to be accomplished by the heat is two-fold.

First, the carbonic acid must be expelled from the calcium carbonate.

Second, the silicate must be decomposed and the finely divided silica converted into lime silicate, soluble in acid.

The first function obviously fixes the lower limit of the temperature to be employed. From the work done by Prof. Hertzfeld, we know that at a temperature of 1050 degrees C the carbonic acid is completely expelled. Lunge and Schochor employed a temperature between 1060 and 1120 degrees Centigrade, their statements being in this respect not as definite as could be desired. However, from the extensive knowledge in existence in regard to the unlocking of silicates by the J. Lawrence Smith process, we know that it is not necessary to carry the temperature very high and this is also not desirable owing to other considerations. The heating must not be carried so high that silicates and aluminates analogous to those of hydraulic cements are formed, reactions which are entirely uncalled for in this process. We thus have the limits of temperature fixed, the lower one being that of the complete expulsion of carbonic acid and the higher one being that of the formation of hydraulic silicates and aluminates.

It was not thought necessary in view of the knowledge available, to undertake an extended series of experiments as to the temperature required. But it was decided to heat a mixture of clay and carbonate of lime at different temperatures and to observe at what temperature the residue

becomes constant. For this purpose a marl, from Harper, Ohio, was selected, whose composition is as follows:

SiO_2	8.75
$Al_2O_3+Fe_2O_3$	2.78
$CaCO_3$	79.22
$MgCO_3$	2.67

A sample of this material was heated to 1000, 1050, 1100 and 1150 degrees Centigrade with the results as shown by the following figures, the time chosen being 90 minutes, that is, the respective temperature was maintained for this time. The amount of marl taken for analysis was 2 grams.

Temperature,	1000°	1050°	1100°	1150°
Residue	2.06	1.00	0.4	...

The results were found to check sufficiently close in other burns. The samples were made up into briquets.

It is thus seen that practically all the marl is decomposed at 1100 degrees. Although this series might have been extended to mixtures of clay and carbonate of lime, yet the fact that this marl, which is acknowledged to be a most excellent cement material, was practically decomposed, together with the data obtained from the work of Lunge and Schochor, Hertzfeld, Smith, seems sufficient to warrant the acceptance of this temperature as the standard.

The question of the time of heating was one that next needed attention. In order to determine the factor of time, the same marl was used and heated for 30, 60, 75, 90 and 120 minutes at the temperature of 1100 degrees C.

Time	30	60	75	90	120 minutes.
Residue	8.6	1.28	0.4	1.0	0.55

In spite of the irregularity in the 90-minute burn, which shows an increase of 0.6 per cent. of the residue, it was decided to adopt 75 minutes as the standard length of the rational burn. The irregularity may be explained by the variations necessarily co-incident with the nature of all rational analyses of this kind, which do not work upon finely pulverized, homogeneous samples.

By 75 minutes is meant the time during which the sample is exposed to the temperature of 1100 degrees C.,

which, of course, means a much longer burn, since one hour elapses before the furnace can be raised to 1100 degrees.

There can be little doubt but that the time thus allowed is ample to accomplish the intended result. It is well known that for the complete decomposition of silicates in the alkali determination, a heating of 50 minutes over a Runsen burner is sufficient, the temperature of which certainly does not exceed 1100 degrees.

Having thus arrived at standard conditions of burning which, in the nature of the case, are bound to be more or less arbitrary, it was decided to apply this method to typical constituents of clay, prepared in several well-graded sizes. For, by knowing the action of lime upon each of these materials and for each of several sizes of grain, it is possible for us to obtain a more definite idea of the character of the more inert constituents of clay, not readily available for hydraulic purposes. Of course, we can say in general that the coarser the particles of quartz and feldspathic and micaceous minerals, the more resistant will they prove to the action of lime, yet we do not know just to what extent the chemical action of lime is affected by the size of grain.

For this reason two series of experiments were carried out in which quartz and feldspar grains were burned together with lime in a mixture containing 80 per cent. of the calcium carbonate, the heat being maintained for 75 minutes at a temperature of 1100 degrees C.

The quartz taken for this purpose was pure quartzite which was crushed and graded to the sizes by sieving. Six sizes were adopted, as follows:

1. Grains between 150 and 120 mesh sieve.
2. Grains between 120 and 100 mesh sieve.
3. Grains between 100 and 80 mesh sieve.
4. Grains between 80 and 60 mesh sieve.
5. Grains between 60 and 40 mesh sieve.
6. Grains between 40 and 20 mesh sieve.

In addition to these sizes, there was included in the series a ground flint, as sold to potters, which is quartzite ground to a fine powder in ball mills using flint pebbles.

After being mixed with $CaCO_3$ in the condition of a slip, the mixture was evaporated to dryness and made into briquettes which were burned.

On making the rational analysis the following results were obtained, as shown by the table:

SIZES	Ground Flint	150-120	120-100	100-80	80-60	60-40	40-30
Per cent. residue.....	28.83	68.8	78.58	86.52	86.27	96.22	96.83
Per cent. taken in solution	71.17	36.2	21.47	13.48	13.73	3.78	3.17

This table shows quite strikingly the effect of size of grain upon the action of lime, and brings out two kinds of facts which have a direct bearing upon the cement industry.

It confirms the general statement that the coarser the grain, the smaller the effect of the lime. Free silica contained in clay, unless very fine, finer than that passing through a 150-mesh sieve, is rendered available only after thorough grinding, which means a certain extra amount of wear and tear of machinery, and a decrease in efficiency corresponding with the coarseness of the quartz.

The second fact is to a certain extent opposite in character to the first. It tells us that it is commercially possible to reduce free quartz to a fineness in which it is available for combination with lime. In order to be more clear, it may be said that of two clays available, one high in alumina and low in quartz, and one with a proper alumina content, but rather high in free silica, the results in regard to the quality of cement will certainly be better when the silicious material is used, in spite of the fact that its grinding is more difficult.

The tendency, only too common, is to work softer clays, which often bring in more alumina, and result in a tendency to "hot" cements, which are toned down by calcium sulphate. Granted that the presence of quartz makes it necessary to grind longer, yet the clay should not altogether be chosen with regard to its easy reduction, but also in regard to the ultimate quality of the cement.

Similarly to the quartz series, the same process was carried through with the corresponding sizes of feldspar grains, which were obtained from several fine specimens of orthoclase crystals, that apparently had not been kaolinized preceptibly.

SIZES.	Ground Feldspar	150-120	120-100	100-80	80-60	60-40	40-20
Per cent. residue.....	3.75	15.45	31.00	64.29	79.63	95.72	....
Per cent. taken in solution	96 25	84.55	69.00	35.71	20.37	4.28	

These results show at once the great difference between quartz and feldspar in regard to being acted upon by lime. It becomes evident that feldspar is a desirable constituent of cement materials. Light-burning clays high in feldspar should therefore receive more attention with reference to the manufacture of light-colored cement.

In addition to these series, commercial potter's kaolin of a good grade was treated by the same process, resulting in a residue of 3.07 per cent., 96.93 per cent. being taken into solution. This verifies the work of the German investigators, who found that clay substance was easily unlocked by lime.

Finally a synthetic mixture, having the following composition, was prepared with the intention of closely imitating such a clay as is commonly found in the alluvial or glacial clays of the country, and as is likely to be tried as a source of silica and alumina in cement manufacture.

Commercial, washed Kaolin	50 per cent.
Feldspar, 150–120 mesh	10 per cent.
Feldspar, 120–100 mesh	10 per cent.
Quartz, 150–120 mesh	10 per cent.
Quartz, 100–80 mesh	10 per cent.
Quartz, 60–40 mesh	10 per cent.
	100 per cent.

This mixture would have a composition on analysis roughly as follows:

SiO_2	65.50
Al_2O_3	24.00
K_2O	3.00
H_2O	7.50
	100.00

Upon being heated with lime it showed, on analysis, a residue of 31.36 per cent., 68.64 per cent. having been taken into solution. The residue, calculated from the previous experiments, should be 30.72 per cent., which is a practical check on the previous work.

The foregoing method has yet to be subjected to the final test of practical application. Not until we have demonstrated that the clays which this method condemns, are also condemned after actual use as cement materials, and that clays which this method endorses as good, are actually found good in use, can we treat it as an established and valuable thing.

However, its claims to usefulness rest on broad and secure technical reasoning. No one can successfully dispute that clays, which give up their silica most readily to the action of lime, are, other things being equal, the best for making cements. Neither can any one affirm that any other method yet proposed or used, has been successful in showing any close relation between the amount of sandy minerals in a clay and its value to the cement-maker.

The clay must contain other sources of silica than that of kaolinite, if it is to be serviceable to the cement-maker, and, in addition, these sources must be such as will readily yield it up. The older methods have stopped after showing that the requisite silicate matter is present. This proposed method is designed to go one step farther, in showing not only that it is present quantitatively, but also whether it is qualitatively suitable as well. It shows whether the silicate matter which is present, is in a form in which it will be readily assimilated into the calcium silicates to be formed, or whether it is in such physically unfit condition that only a limited part of it will be useful.

In the latter case, using the data furnished by the older methods, the cement-maker is likely to produce a cement

too poor in combined silica. Or, if he recognizes this danger, and uses an excess of clay to bring the proportion of available silica up to the proper point, he runs into the counter difficulty of bringing in too much alumina.

When the experimental work, which the Survey has begun, is completed, it is hoped that further data can be given, showing the value of this process in actual work.

DISCUSSION.

N. B.—The paper was read by the Secretary, in the absence of the writer.

Mr. E. C. Stover: Professor Orton has probably been familiar more or less with these experiments, and I would like to ask him a question. In making the burns, was the product colored by soot, or was it burned free from discoloration?

Professor Edward Orton, Jr.: The lime silicates which have been produced so far have none of them blackened until they reached the cement-forming stage. I don't think they get any black coloration because of carbon taken up from the flame. The burn is of a very intense nature, and almost bound to be reducing in character, and it will almost necessarily bring all iron in the cement into the ferrous condition. If we could get a muffle up to that temperature, it would be more easily possible to produce oxydizing conditions at the same time. But, you have got a mixture of sixty-five per cent. of calcium oxide to combine with only twenty-five per cent. of silica, and it takes a higher heat than can be attained in any ordinary muffle. I think the dark color of the clinker produced, is due to the reducing character of the burn and not to carbon.

Mr. Stover: Can you tell what heat has been used? What cone it is equal to?

Professor Orton: A German investigator recently tried a cone test in one of the modern rotary cement kilns. He made a cannon-ball arrangement out of fire-brick, making a hollow ball-shaped affair in two pieces fitting them together so they would clamp or key together; inside was a number of cones. They started this spherical fire-brick

bomb in at one end of the rotary, and let it go through and come out at the other end. It took about an hour and a half to go through, and cone eighteen was melted, I believe.

A friend of mine put some cones into a small stove apparatus he was using for burning cement; he put in numbers five, ten, fifteen and twenty. Ten was melted to a perfectly clear glass; fifteen was *melted into an opaque white slag;* and cone twenty was bent and twisted, but on breaking it to examine the cross section, was found to be only nicely vitrified. This little test shows that a temperature between cones 15 and 20 was reached in burning cement in an experimental way.

Mr. Stover: That explains why I could not make cement. I made one or two cement tests in my biscuit-kilns, and as we fire at cone eight or ten in Trenton, we could not get sufficient heat to make a clinker. The material came out spongy and unvitrified.

THE CLASSIFICATION AND NOMENCLATURE OF THE SILICATES.

BY

EDWARD ORTON, JR., E. M.

One year ago, a very able paper was read before this society on the subject of "Monosilicate Glazes." Simple as this title is, it is somewhat strange that it should have occasioned or permitted any misconstruction or misunderstanding, especially among a body as well acquainted socially and professionally as is the case with members of this society. But, such is the unfortunate condition of chemical technology at this time, especially of the ceramic branch, that some question did arise, as to what this title meant. Some thought that the monosilicate referred to meant a silicate containing one base only; others thought it meant a silicate computed by the oxygen ratio; others construed it in the sense in which the author really used it.

The citation of this illustration must not be construed by any one as a reflection on the choice of this title, or the propriety of its application. In fact, no other could have been used, which would not, in this society at least, have been open still to the same misconstruction. It is cited merely as a fresh instance of the fact with which all of us are familiar, and which is a constant source of trouble and embarrassment to some of us at least, viz.: that the various branches of chemical technology are using different systems of silicate nomenclature.

This confusion arises from several causes. To begin with, the naming of the silicates is really a matter which falls to the chemist. To him belongs the first right to discuss this problem, and from his conclusions we should be slow in departing. It is through the chemist that we have

received our views. They are the pioneers in bringing the degree of order which exists at present, from the chaos of preceding years, and naturally they are factors whose views in the settlement of such questions cannot be ignored.

But chemistry is a very rapidly growing science. It has only been taught in the colleges with any approach to fullness and thoroughness for some fifty years, and the number of trained chemical observers and thinkers has probably increased an hundred fold since 1870; at least, this is true for the United States. Moreover, chemistry is a science of infinite detail. The facts which make up the sum of modern chemical knowledge are simply legion, and it still seems as if our knowledge is capable of infinite extension in the same direction. The mass of recorded information has grown to be so great, that no man's life-time is sufficient to make him acquainted with it, and yet we seem as far from the end of experimental research and deduction as we were twenty-five years ago. In fact, at no time in the past, have such an army of trained, cultured observers been at work, as those that are now eagerly probing into the unknown portions of the chemical field.

As is natural in a rapidly-growing science, our idea of the philosophy of chemical combination, of quantivalence, and of structure, have changed many times, and doubtless will change still further. The chemist of fifty years ago read his formulæ quite differently from the way that we now employ; his system of atomic weights were different; his conceptions of chemical union were different. He knew nothing of the periodic law, of ionization, or of physical chemistry in general. And while the change has been for the better, and while the present conception of chemistry affords a much more comprehensive view than ever before, it is too new as yet to have been adopted in all of the branches of the chemical industries.

Herein lies the trouble. The advance of pure chemistry has been more rapid than that of the technical industries, which depend upon it. The bold outlines of chemistry, and the fundamental method of doing things, have not changed materially in these fifty years. Men knew how to

make certain chemical products as well then as we do now; hence, this work has been relegated to a class of men whose tastes lie in doing well the things they understand, rather than in investigating unknown fields, or busying their minds with speculations on the philosophy of the subject. In other words, the investigator has out-stripped the technicist, and from this fact grows the present confusion.

THE MINERALOGICAL CLASSIFICATION.

Mineralogy is one of those branches of chemical study that have been largely created during these fifty years, and its evolution has followed and in a measure kept pace with that of pure chemistry.

But the mineralogist is not merely a chemist. He has other problems on hand, which sometimes seem to him more important than the chemical constitution of his minerals. The latter are often curiously complex, and chemical analyses have been most unsatisfactory as a means of solving them; while physical means, principally the microscope, polariscope, and sectioning lathe, have shown up methods of attack which have proven much more fruitful. Hence, the mineralogist, while in the main a good chemist, has generally been somewhat behind hand, and his conceptions of mineral structure have not always expressed the latest chemical views. For instance, Dr. Bruno Kerl, in his "Thonwaaren Industrie," cites formulæ for kaolinite from various preceding mineralogists, which give a half dozen different constructions to this simple mineral.

At one stage of their work the mineralogists used a very complex system of formula-writing, which cannot here even be reproduced without the manufacture of special type for the purpose. It is certainly true that no one, possessing merely an ordinary knowledge of chemistry, would understand at first what one of these old mineralogical formulæ meant.

The mineralogist has, for a long time, been puzzled to explain the apparent anomalies in mineral composition. For instance, he finds a number of minerals, all of the same crystaline system, all having some characteristic physical

points in common and yet a most annoying divergence in composition. One, only, of the group may perhaps have a simple chemical structure in which one base combines with one acid. Others will show two bases, the sum of whose equivalents equal the first base in the simple type. Others again will show three, or even four bases, the sum of whose equivalents is merely enough to saturate the original quantity of acid atoms.

For instance, let me quote the composition of some of the pyroxene group.

TABLE I.

NAME	New Chemical Formula	Old Method of Writing Same Formula.
Pyroxene type.....	$RSiO_3$	RO, SiO_2
Malacolite.....	$CaMgSi_2O_6$	CaO, MgO, 2 SiO_2
Malacolite, var.	$CaMg_2Si_2O_8$	CaO, 2 MgO, 3 SiO_2
Sahlite.....	$CaMgFeSi_2O_8$	CaO, MgO, FeO, 3 SiO_2
Sahlite, var.	$Ca_3Mg_3FeSi_7O_{22}$	3 CaO, 3 MgO, FeO, 7 SiO_2
Sahlite, var.	$Ca_2Mg_2FeSi_5O_{15}$	2 CaO, 2 MgO, FeO, 5 SiO_2
Sahlite, var.	$Ca_4Mg_3FeSi_8O_{24}$	4 CaO, 3 MgO, FeO, 8 SiO_2
Hedenbergerite.....	$CaFeSi_2O_6$	CaO, FeO, 2 SiO_2

There are also Lime-Iron-Manganese-Zinc Pyroxenes or *Jeffersonite*, Alumina-Lime-Magnesia Pyroxenes or *Leucaugite*, Alumina-Lime-Magnesia-Iron Pyroxenes or *Augite*, and others whose composition is still a matter of doubt, because it seems impossible to obtain two samples of a composition uniform in all details. At the conclusion of this list, the author (Dana) naively remarks that these groups shade so insensibly, one into another, that it is difficult to classify them accurately.

The hopelessness of finding any rational explanation to this chaotic profusion of chemical compositions, in minerals which have almost everything else in common, was only increased by the attempts of chemists to have mineralogists adopt their newer nomenclature as shown in column 2 of the above table. They were asked to give up the detailed and analytic formulæ in column 3, because these no longer represented the newer conception of the methods of chemical combination. It is easily apparent that the change was not one which conduced to a clearer understanding of

structure, as shown in these complex minerals. The mineralogists loyally attempted to do it, but the entire lack of the graphic character in the new nomenclature has stood as a permanent bar to progress. Dana devised what he calls the "quantivalent ratio" to get around this difficulty, in which he uses the total quantivalence of the basic *vs.* the acidic members of a mineral compound. But this device does not really meet the fundamental objection of the chemists at all; it is only a parallel way of doing exactly what the oxygen ratio had been before designed to do, and the old fundamental conception of molecular union, instead of atomic union, was still there in unchanged strength. In fact, *it had to be there*, to enable the mineralogists to work. Without it, they were lost, for the new method gave them at that time nothing in its place.

A great change has begun to take place in recent years in this matter, through the attempts of mineralogical chemists to apply to inorganic salts, especially the silicates, the same sort of graphic formulæ which have long since proven so valuable in organic work.

Dr. F. W. Clarke's luminous exposition on "The Constitution of Silicates" (U. S. Geol. Survey Bull., 125,) is a most valuable example of what is being done in this line. Here, the silicates are all considered as salts of the five known silicic acids, and by means of the graphic structural formulæ, it begins to be more clearly seen how a mineral may present so kalideoscopic a composition, while all the while retaining a type structure and definite mineral characteristics.

THE METALLURGICAL CLASSIFICATION.

Metallurgy is another of the branches of science which also deals extensively with this subject of silicates, but from a very different point of view. In fact, some people do not like to hear metallurgy classed as a science; they call it an art instead. Without going into that ancient quarrel at this time, it certainly may be said that pure science has greatly profited by the work which has been done in the field of metallurgy. Metallurgy is utilitarian at foundation, without a question. It is not science for science sake, which domi-

nates its followers. The effort of metallurgists is rather to use science so as to make it the handmaiden of industry.

The metallurgist has done much in assisting the chemist to a better understanding of silicate structure, because he has approached it from a new point of view. The mineralogist deals principally with definite crystaline or crystalizable compounds. He has been engaged in discovering just what elements the various inorganic substances, which he finds in nature, contain; and in what relationship they stand to each other. But, back of all of his study and uncertainty is the strong conviction, given him by the chemist, that his minerals are always definite structures, and that it only remains to him to secure pure specimens to find their formula becoming rational.

Not so with the metallurgist. His work lies in tearing down mineral forms, not in seeking to build them up. He knows mineralogy, it is true, because to that field he must go for his crude supplies. But to him, the mineral is interesting principally as an ore, from which he can produce a metal. He studies it to see how much metal it contains, and next with what other worthless material it is associated. To separate these two portions, by physical or chemical processes, is the work of the metallurgist.

In the commoner phases of the work, such as iron or lead smelting, it is the custom to reduce the whole body of the ores to a fiery liquid silicate, of the most fusible sort attainable with cheap and easy accessible fluxes, accompanied with such violent reducing action that the easily reducible oxides will come down to metallic form. From the fluid mass, the metal separates out by gravity, accumulating at the bottom of the fiery pool, while above it, floats the slag composed chiefly of silica and the easily reducible oxides, such as the earths, alkalies and alumina.

These fluid silicates or "slags" of the metallurgist offer, in some respects, an analogy to the fluid fiery magmas of which our earths whole crust was once formed and of which we still have experience in occasional volcanic outbreaks. They differ however in two important respects: (1) they are generally more basic than natural lavas, being com-

pounded so as to give the greatest fusibility and fluidity at the temperature at which the smelting is to be done; and (2) their usefulness is generally complete when the metal has been reduced from them and they then are allowed to cool as fast as possible in the open air. Hence, they have small opportunity for the formation of crystaline minerals, as the real lavas do, which flow out in such enormous mass as to retain their heat for years or even centuries, and which offer every advantage for the gradual growth of crystaline forms.

Nevertheless, a number of distinct crystaline minerals have been recognized and determined in metallurgical slags; enough to show the similarity of the processes of lava and slag formation, and to illustrate the development of mineral forms from slowly cooling fluid silicates.

The slag is therefore to the metallurgist, merely a convenient and effective way of separating off the earthy or silicious gangues of his ores; and since so many and such important processes are thus executed, great study has been devoted to the principles of slag formation or the fluxing of ores. The metallurgist, all unhampered by such considerations as have influenced the mineralogist, in his studies of the relation between the chemical composition or slags and their fusibility and reducing qualities, has developed a system of calculation and comparison which offends the sense of propriety of the stickler for pure science in the chemical field. He finds that certain bases with certain acids make good fusible slags, if used within certain ratios. He finds further, that it is generally better to use several different bases to a given amount of acid, than to depend on the fluxing value of any one base alone. He finds that the easiest and most natural way to maintain these proportions is by the use of ratios, and in order to avoid the constant use of equivalent weights, when different oxides are being used, he makes his calculation on the contained oxygen of the elements.

The oxygen-ratio was not devised by metallurgists, but its convenience and value as a system of comparison and control, early commended itself to him and is now firmly en-

grafted in his work. He classifies silicates by their oxygen-ratio : A protosilicate is to him, one in which the oxygen from all of the bases equals that from all of the acids. In a bisilicate, the acid oxygen is double that derived from the bases.

If all metallurgical products were either proto or bi-silicates, or of some other definite ratio, the disagreement between the mineralogical and metallurgical use of the oxygen ratio would be minimized. But, slags are not made with this object in view. They are made to fuse easily, let their composition be what it may; and when duly reduced to their molecular ratios, are likely to come at some irregular fractional ratio, which is neither a proto or bi-silicate, but lying somewhere between them. The chemist, conceiving of definite compounds, dislikes to use an expression indicating a fractional molecule or an uneven ratio, as such are incompatible with the theory of chemical combination. The metallurgist, on the other hand, does not pretend that his slag is a definite chemical compound, but merely a mixture of uncrystalized, inseparable chemical compounds, chilled into the semblance of a natural mineral by sudden cooling. The ratios of the various elements which compose this once fluid mass must be considered, by all chemical evidence, to be in definite proportions in each compound they have formed, but the sum of all molecules of each kind does not by any means form a ratio which indicates chemical proportions for the whole mixture.

The oxygen ratio therefore is the easy, natural method of contrasting such compounds, regardless of its apparent inconsistency with the laws of chemical combination.

THE CERAMIC CLASSIFICATION.

A fourth branch of chemical technology has in recent years been differentiated from these others in the field of silicate study. This newest division of the subject covers our own specialty, the ceramic field. Although the latest to enter the arena, it cannot truthfully be said that it has entered with the fullest training or with the broadest realization of what has been done in other parallel lines.

The field of the ceramist, though well defined in some respects, is closely affiliated with or co-extensive with that of the metallurgist, in this matter of slag or silicate formation. The metallurgist produces ordinarily slags which are so basic that they form stony lusterless masses when cool; crystals ordinarily have no time to grow to appreciable size, but the mass is found to be an aggregation of finely divided crystaline matter when examined under the polariscope. The mineralogist calls this condition "cryptocrystaline."

The ceramist, in dealing with bodies and refractory materials, produces results very similar indeed to the above described phase of metallurgy. He does not produce these wares by fusion; he takes already crystaline minerals, blends them, and by heat merges the identity of one mineral in to another, until a mass of matter is formed, which may be either amorphous or cryptocrystaline, according to the point to which combination has been brought. The higher the burning temperature, the more perfect the vitrification, and the more likelihood of the production of crystaline compounds from the magma of partially fused matter.

But the ceramist is often most interested and concerned in the production of the glassy silicates which form the basis of nearly all of his decorative work. A glass is a substance about which even at this day much misconception exists. It is sometimes spoken of as a "solid solution." It represents an accumulation of silicate matter, whose ingredients are not present in ratios which easily form crystals. For the most part, they are too high in silica, and while a glass is not uncrystalizable, it does not flash readily into the crystaline state, as the basic slags do.

These views are much strengthened by the fact, easily proven by experiment, that a glassy silicate does form definite crystaline compounds if given a long time to cool and also that a stony silicate, if very rapidly chilled from the fused condition, by pressure between cold surfaces, is converted to the glassy state. Thus it is seen that the glassy condition is more or less an accident of cooling, and that the same substance may form cryptocrystaline slags or lavas in one case and glasses in another.

Now, in the field of the ceramist, the glassy silicates are the subject of his principal study. And they present the most difficult and most intangible field of all, from the very fact of their glassy amorphous nature. To study them at all, some method of comparison of composition is necessary as a first step. For this purpose the ceramist uses the fractional molecular formula. He does not like the oxygen ratio of the metallurgist because of its lack of particularity.

The metallurgist in making a slag, need not care particularly whether one base or another is present in the greatest proportion, or whether his slag alters considerably in composition, if it maintains the general type which his operation requires. In short, the slag is by-product, and is only designed to gather up the more or less fluctuating gangue of the ores and convert it into a silicate, which will permit the metal to separate from it and which will itself flow out freely. Sometimes, as in the basic process of steel manufacture, the slag has a distinct chemical office to perform for the metal, but this is the case oftener in refining, than in smelting operations. Consequently, rather large fluctuations in composition may occur, both in make up of the RO fluxes and the total ratios of base to acid, without detriment to the work.

But with the ceramist, who is treating the delicate problems of glaze and body, every little difference or fluctuation of any sort, counts heavily. He is striving to maintain that delicate adjustment or balance between crazing and shivering, which is the nemesis of most potters, and that which is to the metallurgist, a trifling fluctuation in a by-product, is to him a serious alteration in his principal product.

Consequently, the oxygen formula, which expresses merely the crude, gross-ratios, without specification of the details, does not suit his need, and in place of it, he used the molecules from which the oxygen is derived, each expressed in full form.

Molecular ratios and oxygen ratios are both founded on the old conception, no longer admitted by chemists, that chemical compounds are unions of molecnles instead of atoms. Chemists used to write

Calcium Carbonate, CaO, CO_2.

Now, they write

$$CaCO_3$$

and they say that the previous molecules, CaO and CO_2, which were merged in the new compound, no longer exist.

The new theories of chemical combination are unquestionably far better and more far-reaching in the scope than the old ones. No one who has followed the brilliant work of the last generation can doubt this. But, the fact remains, that the old conceptions, while they are not literally true, at least offer a more tangible basis for calculation and expression of the facts in industrial work than anything which the chemists have brought out since.

A calm and dispassionate view of the facts seems to justify the use of these methods of expression, *especially in the realm of the cryptocrystaline or amorphous silicates*, on the ground of their *convenience*. No other ground should be for one moment set up. The chemists are undoubtedly right in criticizing these methods as not to be harmonized with newer and better views of chemical truth. But their newer methods of expression do not yet reach and embrace this field in which we are working, We cannot give up the present method until a better one is open.

In short, the oxygen ratio and the molecular formulæ, both involving the expression of fractional molecules, are to be viewed merely as mechanical aids in the expression of facts, otherwise inexpressible. They owe their value to their graphic character. By no other than a graphic system, can anyone attempt to marshal the intricacies of a series of glazes into a form which can be either understood or compared.

The chemists themselves use graphic methods of expression as freely as any other scientists. The famous benzene ring, which has done such wonderful service in the field of organic chemistry is a conception which could never have been developed and brought to its present security, had it not been for the hexagonal drawing by which it has been represented. This customary figure is simply a visible material aid to an inmaterial conception. Most minds need the stimulus of the concrete, in order to soar

even a little way into the abstract, and it is safe to say that organic chemistry would be several decades behind its present stand, if its votaries had denied themselves the mechanical aid of such things as graphic formulæ in general.

There still remains a topic in this connection in which the ceramists have laid themselves open to just criticism from the other silicate chemists, viz: our nomenclature.

The chemist denies that a chemical compound can still be considered as a union of molecules, and objects to the molecular or oxygen ratio on this account. But, in the older mineralogical and modern metallurgical nomenclature, this objection is much weakened in its force by the fact that in all of those systems, the difference is merely one of interpretation, not of method of classification or nomenclature.

For instance, Dr. Clarke (previously quoted) states that there are five known silicic acids to which all known crystaline silicates can be referred. These he names and formulates as in column I and II of the following table.

TABLE I.

Chemical Name	Chemical Formula	Molecular Formula	Oxygen Ratio
Ortho-Silicic Acid.....	H_4SiO_4	$2\,H_2O,\ SiO_2$	1 : 1
Meta-Silicic Acid.....	H_2SiO_3	$H_2O,\ SiO_2$	1 : 2
Tri-Silicic Acid.....	$H_4Si_3O_8$	$2\,H_2O,\ 3\,SiO_2$	1 : 3
Di-Ortho-Silicic Acid...	$H_6Si_2O_7$	$3\,H_2O,\ 2\,SiO_2$	3 : 4
Di-Meta-Silicic Acid....	$H_2Si_2O_5$	$H_2O,\ 2\,SiO_2$	1 : 4

In columns three and four are shown the old way of viewing these compounds, which, apart from all ideas of chemical truth, has in the minds of the old school at least, a much clearer and more comprehensible appearance than the new nomenclature.

But, the oxygen ratio of the metallurgist and older mineralogist had no quarrel with the present methods in the results of its classifications. The ortho-silicate of today is the uni-silicate of yesterday. The tri-silicate of today has always been so called. The oxygen contents of the positive and negative sides to each compound, formed a reliable guide

to its classification by the newer chemical basis as well. A uni-silicate by one is a uni-silicate by the other.

But, the ceramist has drifted off into the erroneous habit of naming a silicate by the number of molecules of acid and base respectively. In the following table are given four minerals, which are treated by the different methods of classification:

[See table on next page.]

From this table, it is clearly evident that the ceramic method is founded upon an inaccurate conception and ought not to stand.

In short, the method of displaying a glaze composition in RO, R_2O_3, and acid molecules is most useful to the ceramist on account of its graphic character. But it should not, as was occasionally the case in the writings of Seger and most others who have followed him in this field, be the basis of an attempt to classify or name the silicate itself. The latter must proceed from considerations of its real chemical valence, of which the quantivalent ratio of Dana or the oxygen ratio of the metallurgist are the convenient working expressions.

An example of the misconceptions, which the lack of some co-ordinating influence among these allied branches of industrial chemistry has permitted, can easily be given. The metallurgist has said for years past, that the area of most fusible slag formation lies between the proto and bi-silicates. Seger, in the ceramic field, has told us that the glasses and glazes lie between the bi- and tri-silicates. At first sight, a beautiful connection seems apparent, and it appears that one merely steps from the realm of slags directly into that of the glassy silicates, by passing from one stage of acidity to the next

But, plausible as this theory seems, many phenomena arise which do not seem to fit with it.

The timely paper of Professor Binns, calling attention to Seger's lapse in this matter, was the means of bringing out the fact that real glasses, which Seger correctly says, fall between RO, 2 SiO_2 and RO, 3 SiO_2, represent the extreme

TABLE II

Comparison of Systems of Silicate Nomenclature.

Proper Name of Mineral	Chemical Formula	Chemical Name	Mineralogical Classification Quantivalence of Base vs Quantivalence of Acid	Metallurgical Classification Oxygen in Base vs Oxygen in Acid	Ceramic Classification Molecule of Base vs Molecules of Acid
Zircon	$ZrSiO_4$	Zirconium Ortho-Silicate	Uni-Silicate of Zirconium 8 : 8	Proto-Silicate of Zirconium 1 : 1	Mono-Silicate of Zirconium ZrO_2, SiO_2 1 : 1
Willemite	Zn_2SiO_4	Zinc Ortho-Silicate	Uni-Silicate of Zinc 8 : 8	Proto-Silicate of Zinc 1 : 1	Sub-Silicate of Zinc $2\,ZnO$, SiO_2 2 : 1
Xenolite	$Al_4Si_3O_{12}$	Aluminum Ortho-Silicate	Uni-Silicate of Alumina 24 : 24	Proto-Silicate of Alumina 1 : 1	Sesqui-Silicate of Alumina $2\,Al_2O_3$, $3\,SiO_2$ 2 : 3
Wollastonite	$CaSiO_3$	Calcium Meta-Silicate	Bi-Silicate of Calcium 4 : 8	Bi-Silicate of Calcium 1 : 2	Mono-Silicate of Calcium CaO, SiO_2 1 : 1

upper range of acidity in the series of manufactured silicates, and that an intermediate group, the glazes or glassy silicates in which alumina is a constant factor, come in between the true glasses and the slags. These glazes, as was pointed out by Binns, may be of a much lower oxygen ratio than Seger assigned. We now know that they occupy intermediate ground, some of them falling as low as proto-silicates, the best falling in the region of the bi-silicates, and the harder varieties going up still to the tri-silicate ratio. Above and beyond this come the non-aluminous glasses, with oxygen ratios of 1 : 4 or even 1 : 6.

We now can see much more clearly how it is that acid slags may easily, by sudden cooling, pass into the glassy condition, and also why basic glazes, by too slow cooling, become dead or matt, by assuming the cryptocrystaline condition of slags. And in the transitional stage from the area of the glazes to that of the glasses, we can see how the acid glazes become spoiled by the crystalization of minerals from their matrix, in the slow cooling of the potter's kiln, which would be perfectly free from defect, if cooled quickly like the glasses. And, also, we see how the non-aluminous glass, with its extremely silicious nature would infallibly crystalize, if it were not cooled down below the crystalizing point in a moment after leaving the glass pot.

In summing up, it may be said:

First. That ceramists and metallurgists should unhesitatingly adopt the work and views of the modern school of chemistry, in so far as these views are used in interpreting and understanding abstract chemical principles.

Second. That the molecular formula and the oxygen ratio, though based upon a chemical conception long since abandoned, still afford the best means of studying and exhibiting the composition and properties of the cryptocrystaline and amorphous silicates, with which the methods of expression of the new school of chemists seem still inadequate to deal.

Third. That ceramists and metallurgists are justified, therefore, in the continued use of these methods of expression, until one more in consonance with existing chemical

theory and equally convenient in practical use is brought forward.

Fourth. That they should at all times bear in mind the fact that these methods are merely mechanical aids to expression, and are not in themselves a correct statement of chemical fact as now understood.

Fifth. That ceramists should give up the careless custom of classifying silicates by their molecular ratios, and adopt instead, that of the oxygen ratio, which system agrees in its results, if not in its methods, with the modern school of chemists.

DISCUSSION.

Dr. W. H. Zimmer: I cannot see why the different positions of the different elements in the structure of a silicate should not have some influence on its properties, just as well as the properties of a hydro-carbon are influenced by its method of combination. If you go back fifty years, inorganic chemistry overshadowed the organic. Since that, organic chemistry has overshadowed the inorganic; and I venture to say that one cause for this is that the organic chemists have paid great attention to the grouping of the elements composing these compounds. And I expect if we pay the same attention to the grouping of our silicates, we will find the way the grouping is done has an equal influence on the general properties of our silicates.

[Dr. Zimmer then drew on the black-board the pictorial diagram of the Benzene ring, and showed how the physical properties of its derivatives depended on the position or point at which the substitution of molecules occurred, fully as much as on the nature of the molecule substituted.]

Mr. E. C. Stover: I would like to ask Prof. Orton to change a formula from a mono-silicate to a bi-silicate formula, or to illustrate the mono-silicate ratio and bi-silicate ratio.

[This was done on the blackboard.]

The Chair: My mono-silicate glazes of last year's paper were meta-silicates; one of base to two of acid, on an oxygen ratio.

Mr. Stover: In reading Professor Binn's paper, some of us did not understand it clearly.

The Chair: In the printing of my paper, an error crept into the formula, the sub-figure three being left out. It was an important point in the paper. According to the methods recommended by Professor Orton, which I am sure are desirable, my mono-silicate would be a bi-silicate.

Professor Orton: (Referring to blackboard.) PbO, 0.5 SiO_2 is a mono-silicate, on an oxygen ratio, having a ratio of one to one. Take one molecule of silica and one molecule of lead PbO, SiO_2, and we have a ratio of one to two, or a bi-silicate. Mr. Binns used an expression having this last general formula, except that his glazes contained alumina also, which made, of course, a corresponding increase in the amount of silica present. Professor Binns gave his glazes as mono-silicates from the molecular ratio; they are bi-silicates from the oxygen ratio. Professor Binns pointed out clearly last year that Seger's statement that a glaze must fall between a molecular bi- and tri-silicate was not true, at least for the class of bodies and glazes he was working upon.

The Chair: As a matter of fact, that has also been brought out in Mr. Fickes' paper, which will be read presently.

THE PERMISSIBLE VARIATION IN THE SILICA AND ALUMINA CONTENTS OF RAW LEAD GLAZES.

BY

WALTER MORGAN FICKES, E. M.

The late Dr. Seger in his article "Ueber Glasurfehler und deren Ursachen" (page 451 Seger's Gesamelten Schriften) makes the statement, without supporting it with any data whatever, that "If a glaze shows itself too fusible, it can be hardened by raising the silica content, and thereby decreasing the content of the fluxes. A limit must be observed in this respect, in that the acidity of the glaze must never reach a tri-silicate and never fall below a bi-silicate."

Professor Chas. F. Binns, of the Trenton Technical and Art School, attempted to produce glazes along these formulæ and failed. His conclusion was (see Trans. Am. Cer. Soc., Vol. II) that the trouble was due to too high an acid content, and he reduced it to what he calls a mono-silicate. In these experiments, the trials were burnt at cone 3 and 8, temperatures which are extremely high for raw lead glazes. And in subsequent work at Ohio State University, it was found that better and more consistent results could be obtained at a lower temperature (cone 02).

Prof. Binns in his summary makes the following statement "There is no doubt that each base requires its own content of alumina, and consequently of silica, but so far as we have gone, and all our ceramic research must have its limitations of application, the mono-silicate ratio cannot be successfully departed from in *raw lead* glazes." The statement of Binns and Seger in themselves are in direct opposition, and it was, therefore, undertaken to reproduce the data of Binn's work at the Ceramic Department of the Ohio State University.

The apparent differences between these authors may be entirely due to the local conditions existing at the places of experimentation, or it may be that a meaning has been taken from Seger's statement different from the one which he originally intended. It is to be greatly deplored that Seger did not give the data from which he deduced this law; in its absence we can only depend on what we know of his methods of experimentation.

At the time Dr. Seger made these experiments, the dry press process was not used to any extent in Germany, and it is probable that he used trials made by a plastic process. Now, in the experiments made by Binns, dry-pressed bodies were employed. This brings in a factor of variation, whose value and influence is as yet little known, and which has not been investigated. It seems to be the general opinion that a porous dry-press body would be much more easily attacked by a glaze than a body made from the same clay by a plastic process; for in the latter case, the body is more compact and closer-grained than in the case of a dry-press body. (See remarks by Mayer and Walker, Trans. Am. Cer. Soc., Vol. II.)

A second factor of great importance is the nature of the body and glaze. If, for example, Seger referred to fritted boro-silicate glazes, his assumption is right, for this type of glaze has a much higher content of acid than the raw lead glazes. Here again we are handicapped by the lack of information regarding Seger's experiments. If we assume that the temperatures employed by Seger were such as are ordinarily used for lead glazes (cone No. 05 to No. 1), it is quite evident that we should expect some differences in the results of Seger and Binns, for the latter conducted his work at from cone 3 to 8.

And again, it may be that Seger referred to the composition of the glaze after burning, and not to the raw mixture. This is possible, but not probable.

It may, therefore, be concluded that the differences between the statements of these two writers may be due to variation in conditions which they have not stated, or their work may have been directed toward entirely different portions of the great field of lead glazes.

The general problem which the work of these two preceding investigators suggests is the determination of the limits of composition, within which good raw lead glazes may be produced. Every one knows that if too much silica be added to a glaze, it becomes crystaline, and useless as a glaze. The question is, how much? Seger says between a bi-silicate and a tri-silicate. Binns says not only that a mono-silicate will work, but also that its ratio cannot be departed from if the best results are to be obtained.

In settling this matter of how acid a glaze may be, we at once come into contact with another fact, viz: That it will depend on how much alumina is present with the silica.

Every one knows that alumina tends to prevent the devitrification of a glaze, and also to control its fusibility. Hence, we see that more silica may be used, if more alumina is also used to offset it, but that the resultant mixture must have more heat to make it mature as a glaze. So we see that the field opening up is so extensive and the factors causing variation are so numerous and important, that an examination of the whole field is impossible under present limitations. Therefore, we can only hope to accomplish definite results in this thesis, by restricting ourselves to a very small portion of the whole problem.

FACTORS OF VARIATION

If we would undertake to investigate the entire field, the following factors of variation would have to be considered:

First. Variation in nature and amount of the substances making up the RO elements. For example, taking lead and lime as the base forming materials, we might have all combinations from no lime up to all lime. This same would be true of any two materials, such as soda, potash, lead, baryta, magnesia, etc. In addition to all these combinations, we would also have an almost infinite number of other combinations by using three or more substances. Then again, any change made in the alumina and silica would necessitate a duplication for each of the above combinations. It is, therefore, evident that this investigation must be restricted to a very few simple combinations.

Second. Variation in relation of parts.

(*a*) Variation of oxygen ratio.

By the oxygen ratio is meant the ratio between the oxygen of the base forming elements *i. e.* (RO and R_2O_3), and the oxygen of the acid forming elements, (SiO_2 and B_2O_3).

For example:

$$1.0\ RO,\ 0.15\ Al_2O_3,\ 1.50\ SiO_2$$

$$\left.\begin{array}{l}\text{Oxygen in RO} = 1.00 \times 1 \text{ part } O = 1.00\\ \text{Oxygen in } Al_2O_3 = 0.15 \times 3 \text{ parts } O = 0.45\\ \text{Oxygen in } SiO_2 = 1.50 \times 2 \text{ parts } O = 3.00\end{array}\right\} = 1.45$$

$$\text{The Oxygen Ratio is } \frac{3.00}{1.45} \text{ or } \frac{2.07}{1} \text{ or } 1 : 2.07$$

The oxygen ratio of a glaze can be made to vary by varying either the silica or alumina or RO elements.

(*b*) Variation in the molecular ratio of the alumina to the silica.

By this is meant the ratio between the number of molecules of alumina and the number of molecules of silica. For instance, take the glaze

$$\left.\begin{array}{l}0.7\ PbO\\ 0.3\ CaO\end{array}\right\} 0.2\ Al_2O_3,\ 2.00\ SiO_2$$

Here, there are 0.2 molecules Al_2O_3, to every 2.00 molecules of SiO_2, therefore the ratio is 0.2 : 2.0 or 1 : 10.

This molecular ratio plays an important part in the control of a glaze, since by it the hardness, tendency to flow or devitrify, etc., are influenced.

Third. Variation in burning temperature.

It is known that lead glazes can be produced at temperatures ranging from cone 010 to cone 8, and, of course, the composition of the glaze maturing at either of these temperatures, or at some intermediate temperature, would vary considerably. Hence it would be necessary to restrict ourselves to one temperature in order that the work should be comparable throughout.

Fourth. Variation of kiln conditions.

(*a*) Mode of setting.

The manner in which the ware is placed in the kiln, whether close together or with large open spaces between,

exercises an important bearing upon the degree of perfection attained. If the ware is close set, there is not enough draft space, and the heat not being carried away from the fire box causes intense local temperature, but the ware is underburnt. If, on the other hand, the ware is set too open, the draft finds little or no resistance and carries the heat through the kiln, without giving time for the ware to absorb it. Hence, if we vary the manner of setting radically from burn to burn, we bring in a factor of variation of great importance.

(*b*) Variation in firing conditions.

The influence of kiln gases through oxidation and reduction is quite well known, and needs no explanation. Under this head would also fall—amount of draft, kind of fuel, thickness of bed of fuel on grate, etc.

(*c*) Maturing period.

It is a well known fact that substances when exposed to a long continued heat will melt at a lower temperature than if subjected to a greater temperature for a short period only. This is especially true of glasses and glazes, and its effect would be quite an important factor, and must be regulated to some standard condition.

(*d*) Variation in cooling conditions.

Previous work in this department has shown that if the kiln were closed tight and a heavy bed of fuel left on the grate bars, the glazes would come out badly scummed by sulphur compounds. But if cooled in a clean pure atmosphere, the same glazes would be good.

This partial list of the factors which may influence the results of an experiment, will suffice to show that the conclusions of no experimenter in this field can be accepted at face value, unless accompanied with a carefully drawn statement showing every condition by which they were influenced. We believe that the differences already quoted would not be found if such statements had been made.

LIMITS ASSUMED IN PRESENT WORK

It is now necessary to indicate just what portion of this great field of investigation the present experiments will touch upon.

The real aim of the work is, to define what range of composition a raw lead glaze may have, as to its silica and alumina contents, and still be a workable glaze. And to narrow the inquiry down, so that it admits of completion in the time available, the following restrictions will be made:

First. RO elements.

We will restrict ourselves to the use of two combinations of RO elements. 1st, (0.7 PbO, 0.3 CaO). This was chosen partly because it was the RO used by Binns in his work, and partly because it is the extreme amount of lime which can be taken into solution readily, in a glaze whose other flux is PbO alone, at the temperature of 02. This produces relatively a hard and easily crystalized glaze. 2nd, (PbO). This was chosen because it is the opposite extreme, *i. e.* it produces the softest and most fusible of raw lead glazes.

Second. Ratios of the glaze.

(*a*) The oxygen ratio was selected as the best basis of comparison of glazes; Seger's bi-silicate and tri-silicate become, when measured by the oxygen ratio, 1 : 4 and 1 : 6 respectively. Prof. Binns' work showed clearly that these ratios are impossible in the class of glazes which he treated and with the conditions under which he worked. A little preliminary work brought us to the same conclusion. On the other hand, Prof. Binns recommends the bi-silicate (oxygen ratio) as the best glaze. We determined to begin with the ratio 1 : 3, which we felt sure was too acid, and come down by stages 1 : 2.5, 1 : 2, 1 : 1.5, to 1 : 1, which latter we felt sure would be too basic for a good glaze.

These ratios were made the basis of separation of our glazes into batches or series, one being made for each ratio and with each RO mixture employed, making ten in all.

(*b*) The molecular ratios.

Seger recommends that the alumina shall be about one-tenth of the silica in an ordinary glaze, *i. e.*, the standard molecular ratio is one to ten. In this series of tests, one object was to determine for ourselves the truth of this proposition and accordingly, the glazes we planned, were made to vary between an alumina-silica ratio of 1 : 5 on one end of the

series up to 1 : 25 on the other. In the first, the glaze was very much higher in Al_2O_3 than any usual glaze and in the other extreme, the alumina became so small as to exercise little influence.

Third. A standard temperature of cone 02 was adopted, as it is the temperature at which the largest number of all lead glazes are required to mature in commercial practice.

Fourth. The kiln conditions.

(*a*) Setting.

(*b*) Firing conditions.

These conditions will be discussed fully under head of Burning.

(*c*) Maturing period.

The kiln was maintained at about the finishing temperature (cone 04–02) for a period of ten hours. This compares favorably with the length of time given in actual kiln work.

(*d*) The ware was allowed to cool in pure atmosphere, free from sulphur gases, etc., by drawing fire and ashes at end of the ten hours' maturing period, and allowing the ware to cool with stack damper wide open.

THE PLANNING OF THE EXPERIMENTAL GLAZES.

To accomplish the purposes set forth in the foregoing, five series of glazes were designed. Each series maintained one oxygen ratio between its total basic elements and its total acid elements, but its proportions of alumina and silica were constantly so changed as to produce successively a series of different molecular ratios. Series No. 43, given on the next page, illustrates the method.

These glazes were applied to small dry-pressed tiles, made from bodies representing the three general types of clay wares, *i. e.*, red, buff and white burning bodies.

From a study of this table, it will be seen that all of the glazes given are extremely soft glazes, and that none of them come within the usual range of manufacturing conditions except possibly the last one or two. However, the acidity of the series is very low, and it was thought necessary to make the glazes cover not only the field of usage, but the field of practicable limits as well.

SERIES No. 43.

Numbers of Glazes.	COMPOSITION OF GLAZES.				Oxygen Ratio. Total Base to Total Acid	Molecular Ratio. Alumina to Silica.
	PbO	CaO	Al_2O_3	SiO_2		
443	0.7	0.3	0.033	0.824	1 : 1.5	1 : 25
442	0.7	0.3	0.043	0.846	1 : 1.5	1 : 20
292	0.7	0.3	0.055	0.875	1 : 1.5	1 : 16
293	0.7	0.3	0.064	0.894	1 : 1.5	1 : 14
294	0.7	0.3	0.076	0.920	1 : 1.5	1 : 12
295	0.7	0.3	0.097	0.970	1 : 1.5	1 : 10
296	0.7	0.3	0.110	1.000	1 : 1.5	1 : 9
297A	0.7	0.3	0.130	1.042	1 : 1.5	1 : 8
297B	0.7	0.3	0.160	1.110	1 : 1.5	1 : 7
298	0.7	0.3	0.200	1.200	1 : 1.5	1 : 6

Five such series of glazes were made, having successively a total acidity of 1 : 1, 1 : 1.5, 1 : 2, 1 : 2.5 and 1 : 3, in which the RO elements were 0.7 PbO, 0.3 CaO and a similar five series in which the only difference lay in the RO, which consisted of PbO alone.

MANUFACTURE.

The extreme members of each series were weighed up, ground wet in a porcelain lined jar mill (9¾″ diameter x 12½″ deep, inside) with a charge of flint pebbles, for one and one-half hours. When ground, the resulting slip was screened through a 150 mesh brass screen and allowed to settle, and the supernatant liquid removed. Moisture determinations were then made, and the amount of dry material per gram of fluid glaze was ascertained. With these data, it was easy to calculate the intermediary members of the different series, for, having the same RO and the same ratio between oxygen of base and oxygen of acid in both extremes, there was only one variable—the relation of Al_2O_3 to SiO_2, which was calculated as follows:

Take, for example, Series No. 43, just given, in which the extremes are No. 298, having the formula,

$$1\ RO,\ 0.2\ Al_2O_3,\ 1.20\ SiO_2$$

and No. 443, having the formula,

$$1\ RO,\ 0.033\ Al_2O_3,\ 0.824\ SiO_2$$

To calculate the amounts of each of these extremes entering into No. 295, whose composition we wish to make

$$1\ RO,\ 0.097\ Al_2O_3,\ 0.970\ SiO_2$$

we proceed as follows:

0.200 Al_2O_3 in No. 298
0.033 Al_2O_3 in No. 443

0.167=difference between extremes.

0.200 Al_2O_3 in No. 298
0.097 Al_2O_3 in No. 295

0.103=difference between 1st extreme and No. 295.

$\frac{0.103}{0.167} = 0.6173$ of No. 443
and (1.—.6173) or 0.3827 of No. 298 } to make 1.0 of No. 295

That is, by dividing the difference between the two extremes into the difference between the first extreme and the glaze to be made, we obtain the (decimal) amount of the second extreme. This method is repeated for each of the other intermediates. The strength of the slips must, of course, be taken into account in calculating the amount of slip of each extreme to be used in making the blend.

After blending, the glazes were screened again through 150 mesh screen to insure perfect and complete mixture. Just before dipping, the glazes were again screened in order to prevent the settling and separation of the heavier materials.

Each glaze was dipped on three types of bodies: (1) White, (2) Red and (3) Buff. These bodies were in the form of tile ($4\frac{1}{4}$ inch by $1\frac{1}{16}$ inch biscuit) made by U. S. Encaustic Tile Co., and represent the three great classes of common clay wares, upon which lead glazes are used. The

buff body corresponds to yellow ware, buff brick, terra-cotta, etc. The red body is similar to that used for red brick and terra-cotta, tile, flower-pots, etc. The white body is like that used for white-ware, white wall-tile, etc.

Three pieces of each color of body were used for each number; one trial being placed in the bottom third of the kiln; one in the middle third; and one in the top third. This was done in order to be certain that each glaze had an equal chance to mature in the kiln.

There was a considerable difference in the hardness of the three different bodies; the white being soft and porous; the red porous, but not very soft; and the yellow hard and less porous yet.

BURNING.

When the trials were dipped, they were placed in shallow tile setters, 8 inches by 8 inches square and 1¾ inches high. Each setter, when resting on its next neighbor, inclosed a space about 7 inches by 7 inches and 1⅜ inches high, in which a 6x6 square tile, or 7 or 8 pieces such as were used in these experiments could be placed. These setters were piled up in stacks as high as necessary and made an excellent arrangement for this work; they were carefully luted before placing in the kiln.

In the first burn the ware was placed in the kiln at the same time with some ware made by Messrs. Cannon and Williams, the whole being piled into six bungs. These bungs practically filled the entire kiln chamber, and left only enough space for draft. This burn produced mature ware.

In a burn immediately following this, and made by Messrs. Cannon and Williams alone, the ware was placed in two bungs with at least six inches open space on each side of bung—other conditions remaining the same. In this burn, the ware was immature. It was suggested that this was due to the fact that the draft carried the heat rapidly through the kiln and did not afford sufficient time for this heat to be absorbed by the ware. Therefore, it was decided to fill up the open spaces with fire-clay bats, broken setters, etc. This plan is followed in burn Nos. 2 and 3, and gave

good mature ware, showing that the suggestion made above was correct.

In burns Nos. 1 and 3, coal and coke were used. It was found after burn No. 1, that many of the glazes were full of crystaline matter, and it was decided to investigate this and see if this was due to absorption of sulphur from kiln gases. To this end, burn No. 2 was made with wood and charcoal. The wood was all dry hickory, ash and hard-woods, being scrap and refuse from the Columbus buggy factory. The other conditions were held as before, and the glazes were practically the same, proving that the scum was due to the materials of the glaze segregating out or crystal-izing.

The influence of oxidizing and reducing conditions is quite marked on lead glazes, and, therefore, it was decided to keep the kiln atmosphere as nearly oxidizing as possible. In burn No. 1, during the last half hour of the maturing, a sharp reducing fire was kept up in hopes of removing any sulphur absorbed by the glazes, and thereby to free them from scum. This, so far as could be observed, had no effect on the scum, and interfered with other white glazes in tne same kiln. In burns Nos. 2 and 3, this was avoided, and equally good results were obtained.

THE COOLING OF THE WARE.

The method of cooling is of almost equal importance with the burning, for previous work at O. S. U. ceramics department has showed that good glazes can be ruined if not properly cooled. In one case, a kiln was cooled with stack damper shut and fire box full of coals and ashes. The ware came out very badly scummed, and could hardly be called a glaze. When these same glazes were burnt at the same temperatures and cooled with the damper open, and all ashes and clinkers removed from ash pit, they gave good, clean surfaces as bright as a mirror. Therefore, the last conditions were adopted for these experiments.

We come now to the consideration of the specific glazes made, and the results of their burning. In order to present the facts in the smallest space, the following tabular arrangement has been devised. The number and formula of each glaze made, its oxygen ratio and its molecular ratio, are given in the left-hand columns. In the right-hand columns, the general character of the glaze is indicated by the words, "good, fair or bad." "Fair" indicates a glaze which has a few imperfections, crystaline separations or other defects, but which is so nearly perfect as to lead to the belief that it is a workable mixture if applied to a body suited to it.

SERIES No. 46.

Oxygen Ratio } 1 : 3

RO Fluxes { 0.7 PbO, 0.3 CaO

Glaze Number	Proportions of Alumina and Silica to 1 RO		Molecular Ratio of Alumina to Silica	Character of Glazes after Firing, Ten Hours at Cone 04-02		
	Al_2O_3	SiO_2		White Body	Red Body	Buff Body
287	0.130	2.085	1 : 16	bad	bad	bad
288	0.156	2.202	1 : 14	bad	bad	bad
277	0.200	2.400	1 : 12	bad	bad	bad
289	0.220	2.720	1 : 10	bad	bad	bad
290	0.340	3.030	1 : 9	bad	bad	bad
291	0.420	3.300	1 : 8	bad	bad	bad

REMARKS.

Every glaze in this series failed to produce a glaze or even a glassy mass. The surface of all of the trial pieces, red, buff or white, had the appearance of white frosting. This proves that for these three bodies, at this temperature and with this RO combination, the oxygen ratio of 1 : 3 is not available, at any molecular ratio of alumina to silica. The molecular ratios 1 : 9 and 1 : 8 were decidedly more glassy and better fused than the others. This indicates that the lack of glassy structure in the others is due to devitrification, and that the high alumina contents was able to partly hold this tendency in check, while the low alumina glazes were not.

SERIES No. 45.

Oxygen Ratio } 1 : 2.5

RO Fluxes { 0.7 Pb O / 0.3 Ca O

Glaze Number.	Proportions of Alumina and Silica to 1 RO		Molecular Ratio between Alumina and Silica	Character of Glazes after Firing Ten hours at Cones 04-02.		
	Al_2O_3	SiO_2		White Body	Red Body	Buff Body
246	0.100	1.625	1 : 16	bad	bad	bad
283	0.125	1.720	1 : 14	bad	bad	bad
247	0.150	1.812	1 : 12	bad	bad	bad
299	0.200	2.000	1 : 10	bad	bad	bad
284	0.230	2 110	1 : 9	bad	bad	bad
285	0.300	2.370	1 : 8	bad	bad	bad
285A	0.390	2.712	1 : 7	better	better	bad
286	0.500	3.125	1 : 6	better	better	better

REMARKS.

White Body.—Numbers 246 to 285 are glassy, fairly smooth silicates, full of suspended crystaline segregations. Number 285A is nearly free from crystalization, but have small bare areas due to the glaze not running well, being too refractory for the heat used. No. 286 shows the same defect more pronouncedly. Both of these show practically no crystalization.

Red Body.—The glazes were practically the same as on the white. No good glazes, showing any promise of being workable at this temperature were found.

Buff Body.—The glazes were decidedly better on this body, than on the red or white. This improvement is probably due to the greater hardness of the buff tiles, causing them to be less attacked by the glazes and hence, making the latter less likely to segregate crystaline matter. The glazes produced between molecular ratio 1 : 16 and 1 : 8 are about equal in perfection of finish, but none are workable.

This shows that under the conditions used in this test. the oxygen ratio 1 : 2.5 is still too high for practical use.

SERIES 44.

Oxygen Ratio } 1 : 2

RO Fluxes { 0.7 PbO, 0.3 CaO

Glaze Number	Proportions of Alumina and Silica to 1 RO		Molecular Ratio of Alumina to Silica	Character of Glazes after Firing Ten Hours at Cone 04–02		
	Al_2O_3	SiO_2		White Body	Red Body	Buff Body
450	0.045	1.132	1 : 25	fair	fair	fair
449	0.059	1.177	1 : 20	good	fair	good
278	0.077	1.230	1 : 16	good	good	good
279	0.090	1.270	1 : 14	good	good	good
242	0.125	1.375	1 : 12	good	good	good
280	0.140	1.420	1 : 10	good	good	good
281	0.170	1.510	1 : 9	good	good	good
245	0.200	1.600	1 : 8	good	good	good
245A	0.250	1.750	1 : 7	fair	fair	fair
282	0.330	2.000	1 : 6	bad	bad	bad

REMARKS.

White Body.—Glazes 450 and 449 are fair in quality, but are marred by several patches of crystaline scum. No. 450 worse than 449. Nos. 278 to 245 are good clear bright glazes, any of them workable at this temperature. Nos. 245A and 282 are neither of them mature, though 245A is a smoother better glaze than the best of the series 45.

Red Body.—In general, the glazes are the same as in the white body, but are less troubled with crystaline scum.

Buff Body.—Badly crazed throughout. Except for this defect (which has nothing to do with the scope of this inquiry) the glazes from 1 : 8 up to 1 : 25 are all good. The two lower members, 1 : 6 and 1 : 7 are immature.

The working range of glazes having this oxygen ratio lies, under these conditions, between a molecular ratio of 1 : 8 and 1 : 16.

SERIES No. 43.

Oxygen Ratio } 1 : 1.5

RO Fluxes { 0.7 PbO 0.3 CaO

Glaze Number	Proportions of Alumina and Silica to 1 RO		Molecular Ratio of Alumina to Silica	Character of Glaze after Firing Ten hours at Cones 04:02		
	Al_2O_3	SiO_2		White Body	Red Body	Buff Body
443	0.033	0.824	1 : 25	good	fair	good
442	0.043	0.846	1 : 20	good	fair	good
292	0.055	0.875	1 : 16	good	fair	good
293	0.064	0.894	1 : 14	good	fair	good
294	0.076	0.920	1 : 12	good	good	good
295	0.097	0.970	1 : 10	good	good	good
296	0.110	1.000	1 : 9	good	good	good
297A	0.130	1.042	1 : 8	good	good	good
297B	0.160	1.100	1 : 7	good	good	good
298	0.200	1.200	1 : 6	bad	bad	bad

REMARKS.

White Body.—Numbers 443 and 442 have small globular, greasy-looking drops floating in the glaze and a little crystaline scum. 442 better than 443. No. 292 similar to 443. No. 293 and 294 growing better. Drop-like appearance and scum diminishing. No. 295, 296, 297A and 297B are all good, brilliant glazes. No. 298 is immature at this temperature.

Red Body.—The crystaline scum appears sooner than on the white, appearing in 295.

Buff Body.—All good glazes, except 298, which is slightly immature and badly crazed.

In this series, glaze numbers 442 and 443 were not made in the same blends as the lower part of series, but were weighed and ground separately. This accounts in part for any irregularities shown by them.

SERIES 47.

Oxygen Ratio { 1 : 1

RO Fluxes { 0.7 PbO, 0.3 CaO

Glaze Number	Proportions of Alumina and Silica to 1 RO		Molecular Ratio of Alumina to Silica	Character of Glazes after Firing, Ten Hours at Cones 04–02		
	Al_2O_3	SiO_2		White Body	Red Body	Buff Body
432	0.021	0.531	1 : 25	bad	bad	bad
433	0.026	0.539	1 : 20	good	bad	bad
434	0.037	0.555	1 : 15	good	good	bad
435	0.048	0.572	1 : 12	good	good	bad
436	0.059	0.588	1 : 10	good	good	bad
437	0.067	0.600	1 : 9	fair	good	bad
438	0.076	0.614	1 : 8	fair	good	bad
439	0.092	0.640	1 : 7	bad	good	bad
440	0.111	0.666	1 : 6	bad	good	bad
441	0.143	0.714	1 : 5	bad	bad	bad

REMARKS.

White Body.—Number 432 ran smooth but was full of crystaline matter in large patches. 433 to 436 were all good clear brilliant glazes. 437 was glassy, but full of segregated matter. 438 was similar to 437 but worse. 439 immature, somewhat rough. 440 rougher and less mature. 441 not glassy at all.

Red Body.—Number 432 badly scummed with crystaline matter. 433 smooth, with smaller patches of scum. 434, ditto. 435, some patches of crystaline matter. 436–440, good, brilliant glazes.

Buff Body.—Not a clear glaze in the lot. 433 and 434 are the best.

In this series, there is a considerable variation in the value of the glazes, depending on the body used. Thus we find on the very soft, porous white body, a tendency for those glazes having a high alumina-silica ratio, to take up alumina from the body, and to form good glazes; while those of lower alumina-silica ratio produce scummy glazes. On the other hand, on the harder red body, it is the glazes of low molecular ratio which are best. And again, on the hard buff body, the glazes attack the body very little, and are unable to form combinations by solution, and hence show the effect of their too great basicity.

SERIES No. 48.

Glaze Number	Composition of Glazes by Molecular Formulæ				Oxygen Ratio Base to Acid	Molecular Ratio Alumina to Silica	Character of Glaze after Firing, Ten Hours at Cones 04-02		
	PbO	CaO	Al_2O_3	SiO_2			White Body	Red Body	Buff Body
448	0.7	0.3	0	0.40	1 : 0.8	1 : ∞	good	bad	bad
447	0.7	0.3	0	0.30	1 : 0.6	1 : ∞	good	bad	bad
446	0.7	0.3	0	0.20	1 : 0.4	1 : ∞	bad	good	bad
445	0.7	0.3	0	0.10	1 : 0.2	1 : ∞	bad	good	bad
444	0.7	0.3	0	0	1 . 0	1 : ∞	good	good	good

REMARKS.

This series was produced after the others, owing to the results developed in series 47, in which it was clearly demonstrated that some glazes with as low an oxygen ratio as 1 : 1 were still good, and therefore that the bottom limit of basicity had not yet been reached.

White Body—448, good glaze. 447, good glaze, though with a peculiar dull appearance. 446, no glaze, full of segregated matter. 445, no glaze, is a vitreous mass. 444, a beautiful bright glaze.

Red Body.—448, good glassy surface but large patches of crystaline matter. 447, good glaze, small patches of scum. 446, 445 and 444 good clear glazes.

Buff Body.—448 is a crystaline mass, not matured as a glaze. 447 the same. 446, is a vitreous mass, not glassy. 445 same, curling away from edges. 444, a good clear brilliant glaze.

The results of this series are entirely anomalous and no explanation or theory can be advanced concerning their erratic behavior. The glazes on the red body seem to show that the RO base can, on a porous open-grained dry-pressed body, take up enough alumina and silica to make a glaze by itself. However, the data are too contradictory and inconsistent to attempt seriously to explain them.

SERIES 49.

Oxygen Ratio } 1 : 3 RO Fluxes } PbO alone

Glaze Number	Proportions of Alumina and Silica to 1 RO		Molecular Ratio of Alumina to Silica	Character of Glazes after Firing, Ten Hours at Cones 04–02		
	Al_2O_3	SiO_2		White Body	Red Body	Buff Body
478	0.130	2.085	1 : 16	bad	bad	bad
479	0.156	2.202	1 : 14	bad	bad	bad
480	0.200	2.400	1 : 12	bad	bad	bad
481	0.270	2.720	1 : 10	bad	best of	bad
482	0.340	3.030	1 : 9	bad	series	bad
483	0.420	3.300	1 : 8	bad	bad	bad

REMARKS.

White Body.—478, material infused along edges; much crystaline matter. 479 better, still crystaline. 480, best glaze of the series, but too much crystaline matter for use. 481, less crystaline, but shows tendency to curl up at edges of cracks in the glaze while green, and these cracks form bare spots after firing. 482, bare spots due to curling. 483, bad bare spots; curling.

Red Body.—General appearance of glazes similar to those on the white body, but smoother and with fewer bare spots. 481 and 482 are the best, but contain too much crystaline matter to permit them to be used.

Buff Body.—478 and 479 show crystaline scum around their edges. 480 shows much undissolved matter, but is better. 481 is the best of the series, having run smooth and containing comparatively little suspended undissolved matter. 482, fairly good glaze but wavy, due to immaturity. 483, enamel-like, wavy.

No good or really workable glazes can be produced with as high an acidity as 1 : 3 oxygen ratio, under these conditions.

SERIES No. 50.

Oxygen Ratio } 1 : 2.5 RO Fluxes } PbO alone

Glaze Number	Proportions of Alumina and Silica to 1 RO		Molecular Ratio of Alumina to Silica	Character of Glaze, after Firing Ten Hours at Cones 04–02		
	Al_2O_3	SiO_2		White Body	Red Body	Buff Body
499	0.043	1.410	1 : 33	bad	bad	bad
508	0.059	1.470	1 : 25	good	good	fair
509	0.075	1.510	1 : 20	good	good	good
470	0.100	1.625	1 : 16	good	good	good
471	0.125	1.720	1 : 14	good	good	good
472	0.150	1.812	1 : 12	good	good	good
473	0.200	2.000	1 : 10	good	good	good
474	0.230	2.111	1 : 9	good	fair	good
475	0.300	2.370	1 : 8	good	bad	good
476	0.390	2.712	1 : 7	bad	bad	bad
477	0.500	3.120	1 : 6	bad	bad	bad

REMARKS.

White Body.—499, full of crystaline matter, edges dry and crystaline. 508 to 475, good clear brilliant glazes. 476 glassy but shows bare spots, due to curling. 477, semi-glassy with large bare spots.

Red Body.—499, similar to same glaze on the white body. 508 to 473 inclusive, are good clear glazes. 474 begins to show crystaline matter. 475 crystaline separations become quite prominent. 476, immature. 477, vitreous, large bare spots.

Buff Body.—Similar to same series on white body. 508 shows a tendency to immaturity.

SERIES 51.

Oxygen Ratio } 1 : 2 RO Fluxes } PbO alone

Glaze Number	Proportions of Alumina and Silica to 1 RO		Molecular Ratio of Alumina to Silica	Character of Glazes after Firing Ten Hours at Cones 04–02		
	Al_2O_3	SiO_2		White Body	Red Body	Buff Body
510	0.000	1.00	1 : ∞	fair	fair	fair
511	0.021	1.063	1 : 50	good	good	good
512	0.033	1.099	1 : 33	good	good	good
461	0.045	1.133	1 : 25	good	good	good
462	0.059	1.177	1 : 20	good	good	good
463	0.095	1.285	1 : 15	good	good	good
464	0.125	1.375	1 : 12	good	good	good
465	0.140	1.420	1 : 10	good	good	good
466	0.170	1.510	1 : 9	good	good	good
467	0.200	1.600	1 : 8	good	good	good
468	0.250	1.750	1 : 7	good	good	good
469	0.330	2.000	1 : 6	bad	bad	bad

REMARKS.

In this series, the glazes with exception of the two extremes, are all good clear glazes on all bodies. The 510 glazes show a tendency to immaturity on the white and yellow bodies. On the red, it shows a little crystaline matter. The aluminous extreme, 469 is not mature on any body, and shows much undissolved matter.

SERIES 52.

Oxygen Ratio } 1 : 1.5 RO Fluxes } PbO alone

Glaze Number	Proportion of Alumina and Silica to 1 RO		Molecular Ratio of Alumina to Silica	Character of Glazes after Firing Ten Hours at Cones 04-02		
	Al_2O_3	SiO_2		White Body	Red Body	Buff Body
513	0.00	0.750	1 : ∞	good	good	good
514	0.015	0.780	1 : 50	good	good	good
515	0.024	0.804	1 : 33	good	good	good
451	0.033	0.824	1 : 25	good	good	good
452	0.043	0.846	1 : 20	good	good	good
453	0.055	0.875	1 : 16	good	good	good
454	0.064	0.894	1 : 14	good	good	good
455	9.076	0.920	1 : 12	good	good	good
456	0.097	0.970	1 : 10	good	good	good
457	0.110	1.000	1 : 9	good	good	good
458	0.135	1.098	1 : 8	good	good	good
459	0.160	1.110	1 : 7	good	good	good
460	0.200	1.200	1 : 6	good	good	good
516	0.241	1.292	1 : 5	good	good	bad
517	0.410	1.670	1 : 4	bad	bad	bad

REMARKS.

All the members of this series produce good glazes, except the two aluminous extremes, 516 and 517. 516 is good on the white and red bodies, but on the buff body the glaze is badly scummed with segregated matter. 517 is immature on all bodies.

In this series, as in the preceding, it is clearly shown that the glazes with very low molecular ratio (*i. e.* relatively high in Al_2O_3) are unable to carry the alumina, and it crystalizes out.

SERIES No. 53.

Oxygen Ratio } 1 : 1 RO Fluxes { PbO alone

Glaze Number	Proportions of Alumina and Silica to 1 RO		Molecular Ratio of Alumina to Silica	Character of Glaze, after Firing for Ten Hours at Cones 04-02		
	Al_2O_3	SiO_2		White Body	Red Body	Buff Body
484	0.021	0.531	1 : 25	bad	fair	bad
485	0.026	0.539	1 : 20	good	good	bad
486	0.037	0.555	1 : 15	good	good	bad
487	0.048	0.572	1 : 12	good	good	bad
488	0.059	0.588	1 : 10	good	good	bad
489	0.067	0.600	1 : 9	good	good	bad
490	0.076	0.614	1 : 8	good	good	bad
491	0.092	0.640	1 : 7	good	good	bad
492	0.111	0.666	1 : 6	good	good	bad
493	0.143	0.714	1 : 5	good	good	bad

REMARKS.

White Body.—All the glazes of this series are good on this body, except 484, which is full of fine crystaline matter.

Red Body.—All glazes are good, though both extremes show they are at the limits.

Buff Body.—None of these glazes are good, being full of separations. The glazes have been unable to take up alumina or silica from the hard buff body and have therefore shown out in their true character as matt glazes, due to basicity.

On the white and red bodies these glazes made very fusible compounds which ran off the surface and stuck the tiles fast to the setters.

SERIES 54.

Glaze Number	Composition of Glazes by Molecular Formulæ				Oxygen Ratio Base to Acid	Molecular Ratio Alumina to Silica	Character of Glaze after Firing Ten Hours at Cones 04–02		
	PbO	CaO	Al_2O_3	SiO_2			White Body	Red Body	Buff Body
498	1.0	0	0	0.4	1 : 0.8	1 : ∞	good	good	bad
497	1.0	0	0	0.3	1 : 0.6	1 : ∞	good	good	bad
496	1.0	0	0	0.2	1 : 0.4	1 : ∞	fair	good	bad
495	1.0	0	0	0.1	1 : 0.2	1 : ∞	bad	good	bad
494	1.0	0	0	0	1 : 0	1 : ∞	bad	good	bad

REMARKS.

White Body.—494, pure lead oxide, did not make a glaze on the white tiles. It formed a dry crystaline mass. 495, with one-tenth of silica, shows a marked improvement, but is still crystaline. 496 is much better, showing little crystaline matter. 497 and 498 are both good, perfect glazes.

Red Body.—All glazes show smooth glossy surfaces, but with low silica, or none, the glaze has attacked the body vigorously, as was shown by the dark spots on the surface, where the glaze was of unusual depth.

Buff Body.—The 494 glaze is a dry vitreous mass. 495, 496 and 497 are all dry crystaline masses, showing an entirely dead or matt surface, with no glassy spots. No. 498 showed some glassy spots, but was mostly covered with segregated matter.

Series 54 is much less inconsistent and contradictory than the other special series, No. 48. It apparently shows that on a hard body, like the buff, a glaze becomes matt through excess of basic matter. On a soft body, like the white tile body, the glaze acts so energetically to supply its silica and alumina from the body, that it is only when practically pure flux is used that the results become valueless. On the red body, not only is silica and alumina taken up, but also the red clay seems to make a better supply of these substances than the more refractory white body, and the glazes are good when nothing but basic matter is supplied, and the silica and alumina all come from the body.

DISCUSSION OF THE RESULTS.

The great mass of data exhibited in the twelve preceding tables do not readily yield the underlying principles of which we are in search, without some more graphic and condensed method of expressing the facts themselves. The following plates, one for each kind of body on which the experiments were made, are designed to so assemble the data as to make their meaning clear and unmistakable to any one.

The charts are drawn so that the abscissae represent the five different oxygen ratios on which these glazes were compounded, 1 : 1, 1 : 1.5, 1 : 2, 1 : 2.5 and 1 : 3, while the ordinates represent the various ratios which the molecules of alumina bore to those of silica, running from 1 : ∞ in glazes 510, 513 and others down to 1 : 4 in the case of glazes 517.

The position of every one of the 108 different glazes made and tested can thus be located on the chart by means of circles. A line drawn around the outside members of these series marks the area of the test.

There were a number of poor and worthless glazes made and, by marking each circle with the character of its glaze on firing on each body, and with each different RO base employed, and connecting the outside members by dotted lines two areas are marked out, which represent the areas respectively of the glazes which are good, using PbO as a base, and with 0.7 PbO and 0.3 CaO as a base.

CHART SHOWING BEHAVIOR OF RAW-LEAD GLAZES ON DRY-PRESSED WHITE TILE

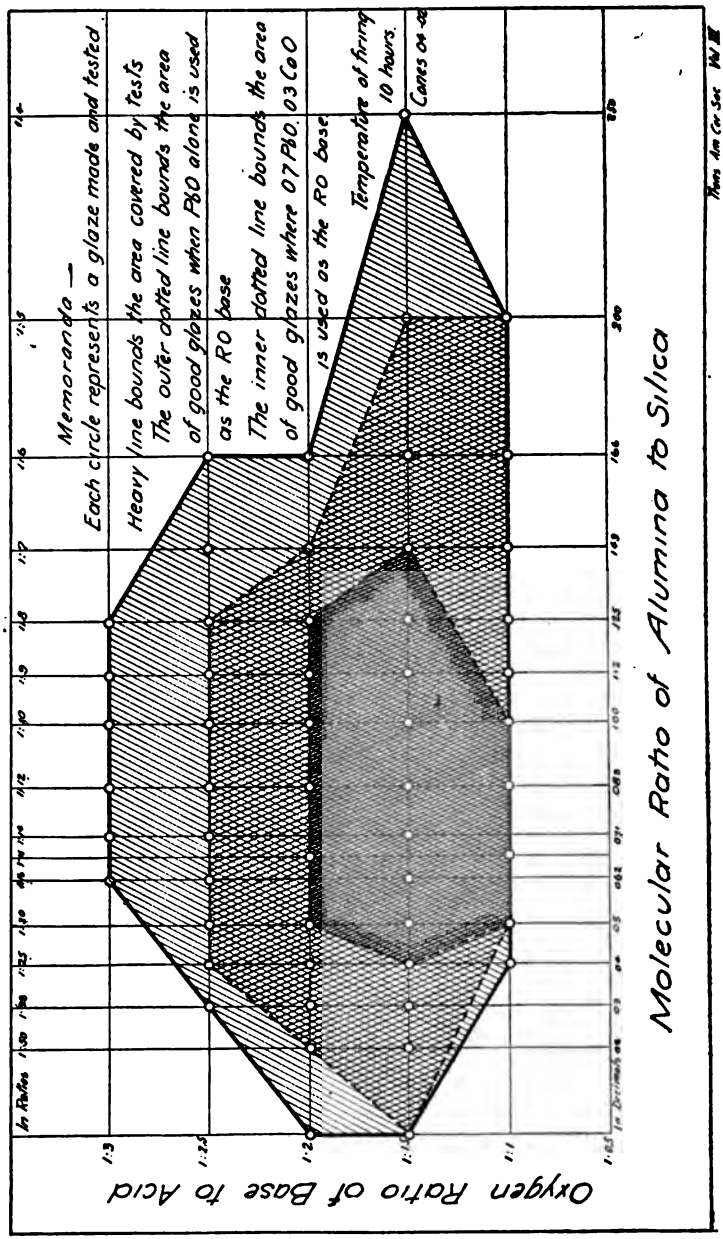
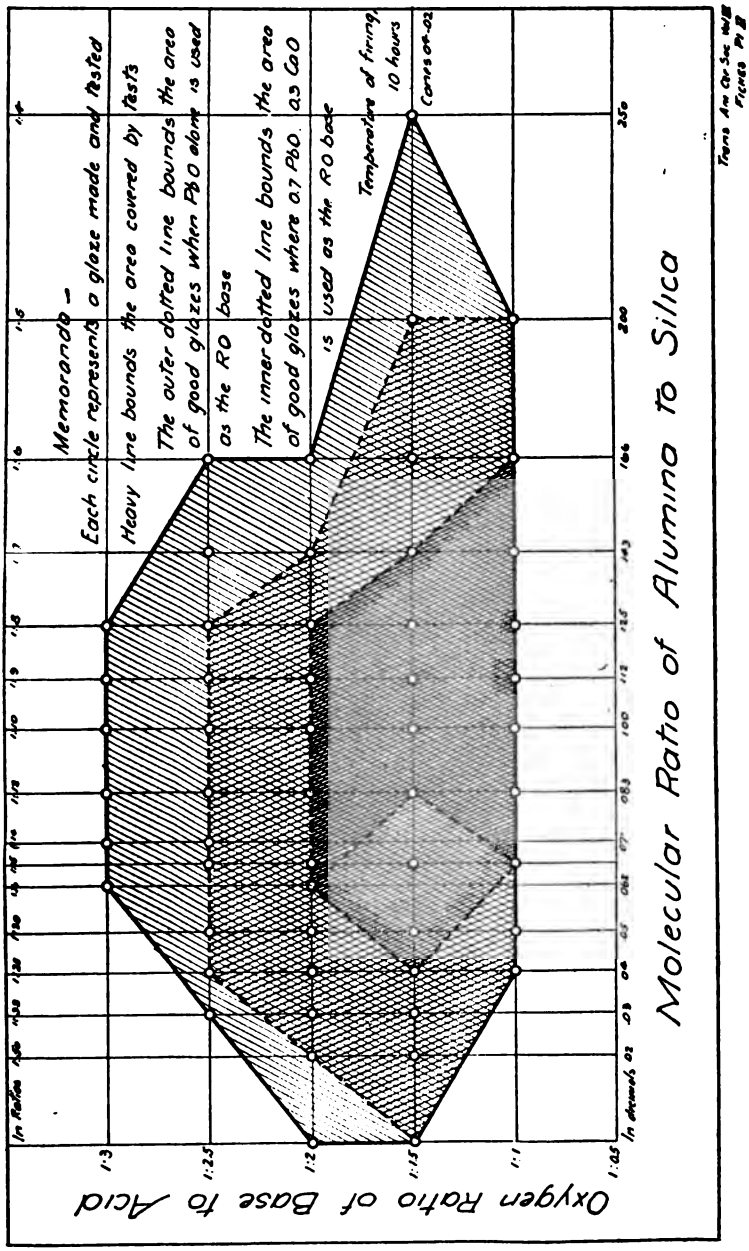


CHART SHOWING BEHAVIOR OF RAW-LEAD GLAZES ON DRY-PRESSED RED TILE



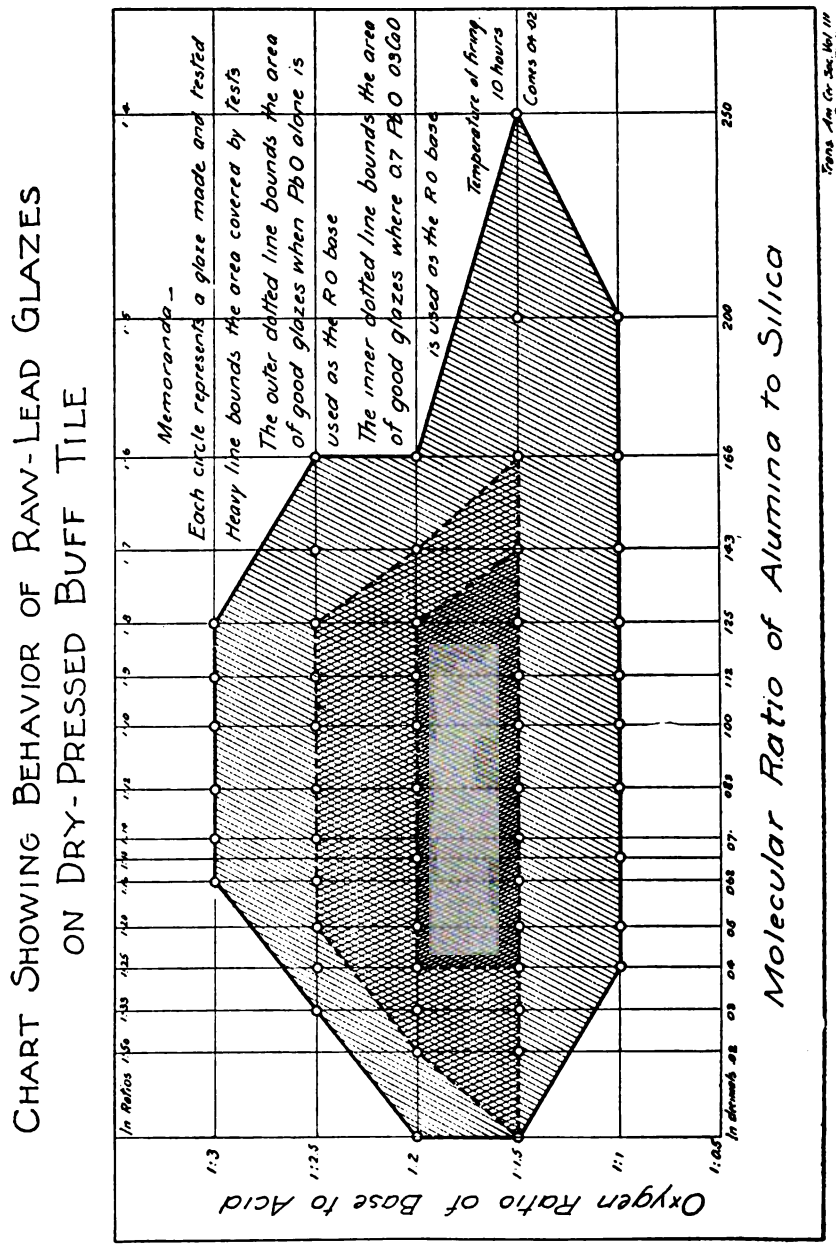


CHART SHOWING BEHAVIOR OF RAW-LEAD GLAZES
ON DRY-PRESSED BUFF TILE
Oxygen Ratio of Base to Acid
Molecular Ratio of Alumina to Silica
Memoranda—
Each circle represents a glaze made and tested
Heavy line bounds the area covered by tests
The outer dotted line bounds the area of good glazes when PbO alone is used as the RO base
The inner dotted line bounds the area of good glazes where .67 PbO .33CaO is used as the RO base
Temperature of firing 10 hours
Cones 04 to 02
1:3
1:2.5
1:2
1:1.5
1:1
1:0.5

It must be understood that a term "good glaze" in this paper refers only to those glazes which run clear, smooth and bright, and does not refer to fitting the body, ability to stand on tilted ware, attacking the body, or to any other point usually entering into the valuation of a glaze by a potter. The assumption in this paper is, that any glaze mixture that will produce a clear, glassy coating would, on a suitable body, be a perfect glaze, and would, under some possible condition, be of commercial value.

From these three plates we may readily see that:

First. The boundaries of the areas of good glazes on the three bodies are similar in shape.

Second. On the two softer bodies, red and white, the good areas are approximately equal. On the hard body, the buff, the area is much smaller.

Third. The area covered by the lead-lime and pure lead glazes producing good results, though somewhat different in size, are nearly similar in shape.

Fourth. No good glazes are produced on any body with an oxygen ratio of 1 : 3.

Fifth. Between the oxygen ratios of 1 : 1.5 and 1 : 2, we have more good glazes than with any other ratios.

Sixth. It is probable that good glazes can be produced with oxygen ratios less than 1 : 1, but this portion of the field has not been investigated.

Seventh. It is probable that the lines connecting the extreme ratios, known to be good, are not straight lines, but curve outward towards the best glazes of the next highest ratios.

Eighth. The molecular ratios from 1 : 8 to 1 : 15, inclusive, show themselves to be in general, on the three types of body employed, the most available working limits, and to be in less danger from undissolved matter or crystaline segregations, than any other ratios, either greater or less.

We may, therefore, conclude that, under the conditions of this series of experiments, the shaded areas represent the limits of composition, using the given RO elements, within which clear, brilliant, smooth-flowing glazes may be expected.

The experiments show in reference to the molecular ratio that:

First. Those glazes having a high (Al_2O_3: SiO_2) ratio, (*i. e.*, which contain only small quantities of alumina) have a tendency, especially when they occur in conjunction with the higher oxygen ratio, to form crystaline spots.

Second. Those glazes, having very low molecular ratios, and which are, therefore, high in clay substance, are unable to take into solution all the silicate matter which is present, and the result is a scummy, unfused appearance.

Series 48 and 54 were made up to find out the effect of the entire absence of alumina from the raw glaze. In one glaze of each of these two series, neither silica nor alumina were added.

Series No. 48 was almost anomalous throughout, and was contrary to all expectations. The glaze, containing only RO (0.7 PbO, 0.3 CaO), derives sufficient alumina and acid from the body to form good glazes on all trials. With 0.1 SiO_2 and no alumina the glaze becomes bad on two trials, but remains good on the red bodies. With 0.2 SiO_2, and up to 0.4 SiO_2, the glazes gradually improve on the white body to good glazes. On the red body, they gradually become spotted with crystaline matter. On the buff body, while not becoming good glazes, they do grow smoother as the SiO_2 increases. I can offer no explanation of this, other than it is due to some difference in the bodies, but what this difference is I cannot say.

In series 54, on the white body, glazes gradually improve from no glaze at all, with lead alone, to good, perfect glazes with 0.4 SiO_2. A similar improvement is noted on the buff body, but no good glazes are found.

The red body shows good, glassy surfaces, but when very little or no silica was provided by the glaze itself, the glazes are marred by dark spots where the glazes have eaten into the body. This series is much more natural than series No. 48, for it shows that the buff and white bodies are unable to supply sufficient alumina and silica to the lower members of this series to form good glazes. On the other hand, the red body is easily attacked by the glazes and gives

up sufficient alumina and silica to form glasses. The buff body is so hard that not one glaze, even 0.4 SiO_2 has derived enough alumina from the body to become perfect.

CONCLUSIONS.

The work done has been very incomplete and, like most investigations, is only sufficient to cause in the investigator a lively desire to do the whole work over again in the light of the experience gained.

The tests should be pushed further, in the direction of glazes of still lower oxygen ratio than 1 : 1, for, on dry-pressed wares, either red or white, it will be observed that a great number of good glazes were obtained. The limits, at the higher acidities, both in the direction of lower and higher alumina, have been probably pretty well defined.

Also the next experimenter should especially conduct his investigation on plastic-clay trial pieces of a fair degree of vitrification. The writer is convinced that the extended area of the good glazes on white and red bodies proceeds largely from the ease with which a glaze is able to supply its wants in SiO_2 and Al_2O_3 from the dry-pressed body.

The buff body, on the other hand, is much more vitreous and close-grained, and resembles a plastic-clay body in texture. On that body, the area of good glazes is much more clearly defined in all directions; this looks as if plastic-pressed, pottery bodies, fairly hard burnt, would permit very much less latitude in the area of possible good glazes.

The remarkable reduction in the area covered by workable glazes, which followed when 0.3 CaO substitutes 0.3 PbO in the RO base is most instructive. At the same time this greatly reduced area is undoubtedly much nearer the ordinary practice of the pottery industry that the very extended field where lead alone is the fluxing base. The latter is not a common condition in the ceramic indstries.

The next experimenter, to take up this field, should try the same general plan, but with other standard and well-known combinations of RO fluxes, such as:

$$\left.\begin{matrix} 0.7\ PbO \\ 0.15\ K_2O \\ 0.15\ CaO \end{matrix}\right\} \text{ or } \left\{\begin{matrix} 0.60\ PbO \\ 0.20\ K_2O \\ 0.10\ CaO \\ 0.10\ ZnO \end{matrix}\right.$$

Notwithstanding the incompleteness of this work, and the many questions which it has opened up without answering, we still think it has pretty clearly established the following facts:

First. That the limits shown in plates I, II and III represent the correct limits, within which will be found all of the good glazes available, under the conditions as to temperature, bodies, burning, etc., assumed for these experiments.

Second. That good, perfect glazes can be produced with other oxygen ratios other than 1 : 2. But that, between the oxygen ratios of 1 : 1.5 and 1 : 2, we have the safest field for raw lead glazes.

Third. That the molecular ratio exercises an important function in controlling the hardness of the glaze and in controlling the tendency to crystalization during cooling.

Fourth. That, between the molecular ratios of 1 : 8 and 1 : 15, there is more probability of forming a good, perfect glaze, regardless of oxygen ratios and RO element, than with any other numbers.

Fifth. That, for each combination of RO elements, composing the fluxing base of a glaze, the limits within which good glazes can be produced, will depend on its fluxing power. The richer in lead, the wider the limits.

DISCUSSION.

Mr. Stover: As I understand, Mr. Binns, this whole series of trials are uniform with what you used last year?

The Chair: Yes, sir.

Mr. Stover: But going farther down the scale?

The Chair: But nothing to deny the results attained last year. In fact, the ratio of one to two of oxygen, which Mr. Fickes recommends, is exactly the same thing as was brought out last year as the molecular mono-silicate.

A NEW GLAZE-FLUX.

BY

WILLIAM CANNAN, Jr.

In the spring term of the school year 1899-1900, at the Ohio State University, Mr. Ira Williams, B. Sc., now instructor in Ceramics at the Iowa State University, and I elected as our experimental work, to be carried under Prof. Orton's direction, the replacement of the oxide of lead (PbO) by the available RO oxides in clear, colorless, raw-lead glazes, maturing between cones 04-02.

The more common RO oxides which form clear, colorless glazes are oxide of lead (PbO), oxide of barium (BaO), oxide of strontium (SrO), oxide of potassium (K_2O), oxide of zinc (ZnO), oxide of sodium (Na_2O), oxide of calcium (CaO), oxide of magnesium (MgO). Oxide of strontium (SrO) was not considered, because it is not on the market in quantity to make it a source of commercial supply at this time. Oxide of sodium (Na_2O) was not used because there is very little soda-feldspar on the market, and even that is not of a uniform composition. The other available salts of sodium are the carbonate of sodium, the chloride of sodium, and the nitrate of sodium, all of which are soluble, and cannot be used with any degree of accuracy without fritting.

Therefore the RO oxides used in these experiments were confined to the oxides of lead, barium, potassium, zinc, calcium and magnesium. These oxides were derived from the following sources: the oxide of lead from commercial white lead, the oxide of barium from commercial barium carbonate, the oxide of potassium from potash feldspar, the oxide of zinc from commercial oxide of zinc, the oxide of calcium from English whiting (calcium carbonate), the oxide of magnesium from commercial magnesium oxide.

THE DESIGNING OF THE SERIES OF GLAZES.

In the construction of the series, we kept in mind Dr. Seger's statements on the fusibility of the RO oxides with which we were to work, which is as follows in the order of their fusibility: oxide of lead, the most fusible, oxide of barium, oxide of potassium, oxide of zinc, oxide of calcium and oxide of magnesium, the most infusible. The fusibility of the RO oxides, according to Dr. Seger, you will notice is as their molecular weight, the heaviest being the most fusible, and the lightest the most infusible. We replaced so much oxide of lead with each RO oxide in turn that we were sure that it would not mature under the conditions assumed for the test, and used this hard glaze as one extreme of each series, and a pure lead glaze as the soft extreme of the same series.

These we followed with series in which two oxides were used at once in replacing lead and then three, four and finally five oxides at once.

The details are explained more closely as follows:

First. We took as the type glaze, one in which PbO is the only fluxing oxide, having the formula PbO 0.2 Al_2O_3, 2.0 SiO_2, which we called No. 300.

Second. We constructed a series for each of the oxides used, in each of which the glaze No. 300, PbO, 0.2 Al_2O_3, 2.0 SiO_2 was the soft end, and the hard end was made according to the oxide used.

The following is an example of one of the single replacement series.

SINGLE REPLACEMENT SERIES.

Glaze No.	Molecular Formulae of Glazes.			
	K_2O	PbO	Al_2O_3	SiO_2
300	....	1.00	0.20	2.00
301	0.05	0.95	0.20	2.00
302	0.10	0.90	0.20	2.00
303	0.15	0.85	0.20	2.00
304	0.20	0.80	0.20	2.00

In glaze No. 304, we introduced all the Al_2O_3 from spar; this was done to get as much K_2O in the glaze as possible.

In each of the single replacement series, the equivalent of the replacing oxide increased by 0.05 equivalent and the PbO decreased by 0.05 equivalent up to the hardest glaze for each different oxide; these extremes, or "hard ends" had the following formulæ:

Lime Lead Series...	0.6 CaO	0.4 PbO	0.20 Al_2O_3	2.00 SiO_2
Magnesia Lead Ser.	0.4 MgO	0.6 PbO	0.20 Al_2O_3	2.00 SiO_2
Barium Lead Series	0.5 BaO	0.5 PbO	0.20 Al_2O_3	2.00 SiO_2
Zinc Lead Series...	0,3 ZnO	0.7 PbO	0.20 Al_2O_3	2.00 SiO_2

Third. We constructed ten "double replacements," or series in which two oxides were used to replace the PbO. In all of these double replacement series, K_2O was used, and was a constant quantity throughout each series. The following is an example of one of these double replacement series.

DOUBLE REPLACEMENT SERIES.

Glaze No.	Molecular Formula of Glazes.				
	K_2O	CaO	PbO	Al_2O_3	SiO_2
301	0.05	0.00	0.95	0.20	2.00
336	0.05	0.05	0.90	0.20	2.00
337	0.05	0.10	0.85	0.20	2.00
338	0.05	0.15	0.80	0.20	2.00
339	0.05	0.20	0.75	0.20	2.00
340	0.05	0.25	0.70	0.20	2.00
341	0.05	0.30	0.65	0.20	2.00
342	0.05	0.35	0.60	0.20	2.00

Three other series similar to this were made, in which K_2O and CaO replaced the PbO, and in each advancing series, the K_2O increased by 0.05 equivalent, but remained constant throughout the series. In these series, the CaO increased by 0.05 equivalent and the PbO decreased by 0.05 equivalent, with each successive glaze. This gave us four series of potash-lime replacements, in which the lead and lime fluctuated in each glaze, and the other elements remained fixed.

0.05 K_2O	CaO and PbO Fluctuating.	0.20 Al_2O_3	2.00 SiO_2
0.10 K_2O	CaO and PbO Fluctuating.	0.20 Al_2O_3	2.00 SiO_2
0.15 K_2O	CaO and PbO Fluctuating.	0.20 Al_2O_3	2.00 SiO_2
0.20 K_2O	CaO and PbO Fluctuating.	0.20 Al_2O_3	2.00 SiO_2

Two series similar to these were constructed for each MgO, BaO, ZnO. The following is an example of each series:

MgO double replacement series:

Low Potash, 0.10 K_2O, MgO and PbO fluctuating 0.20 Al_2O_3 2.00 SiO_2
High Potash, 0.20 K_2O, MgO and PbO fluctuating 0.20 Al_2O_3 2.00 SiO_2

BaO double replacement series:

Low Potash, 0.10 K_2O, BaO and PbO fluctuating 0.20 Al_2O_3 2.00 SiO_2
High Potash, 0.20 K_2O, BaO and PbO fluctuating 0.20 Al_2O_3 2.00 SiO_2

ZnO double replacement series:

Low Potash, 0.10 K_2O, ZnO and PbO fluctuating 0.20 Al_2O_3 2.00 SiO_2
High Potash, 0.20 K_2O, ZnO and PbO fluctuating 0.20 Al_2O_3 2.00 SiO_2

Fourth. We constructed "triple replacement" series, or series in which three oxides were used simultaneously to replace PbO. In all of the triple replacement series, K_2O and CaO were used, and each was constant through each series. The following is an illustration:

TRIPLE REPLACEMENT SERIES.

Glaze No.	Molecular Formula of Glazes.					
	K_2O	CaO	MgO	PbO	Al_2O_3	SiO_2
344	0.10	0.10	0.00	0.80	0.20	2.00
386	0.10	0.10	0.05	0.75	0.20	2.00
387	0.10	0.10	0.10	0.70	0.20	2.00
388	0.10	0.10	0.15	0.65	0.20	2.00
389	0.10	0.10	0.20	0.60	0.20	2.00
390	0.10	0.10	0.25	0.55	0.20	2.00
391	0.10	0.10	0.30	0.50	0.20	2.00

Series similar to this were made in which BaO and ZnO were each in turn combined with fixed quantities of K_2O and CaO, in replacing lead oxide, of which the following schedule gives the key:

0.15 K_2O	0.15 CaO	BaO and PbO Fluctuating.	0.20 Al_2O_3	2.00 SiO_2
0.20 K_2O	0.20 CaO	ZnO and PbO Fluctuating.	0.20 Al_2O_3	2.00 SiO_2

Fifth. We constructed three "quadruple replacement" series, or series in which four oxides were used at once to replace PbO, in which the K_2O and CaO were constant throughout each series.

QUADRUPLE REPLACEMENT SERIES.

Glaze Number	Molecular Formula of Glazes.						
	K_2O	CaO	MgO	ZnO	PbO	Al_2O_3	SiO_2
530	0.10	0.10	0.050	0.050	0.70	0.20	2.00
531	0.10	0.10	0.075	0.075	0.65	0.20	2.00
532	0.10	0.10	0.100	0.100	0.60	0.20	2.00
533	0.10	0.10	0.125	0.125	0.55	0.20	2.00
534	0.10	0.10	0.150	0.150	0.50	0.20	2.00
535	0.10	0.10	0.175	0.175	0.45	0.20	2.00
536	0.10	0.10	0.200	0.200	0.40	0.20	2.00
537	0.15	0.15	0.050	0.050	0.60	0.20	2.00
538	0.15	0.15	0.075	0.075	0.55	0.20	2 00
539	0.15	0.15	0.100	0.100	0.50	0.20	2.00
540	0.15	0.15	0.125	0.125	0.45	0.20	2.00
541	0.15	0.15	0.150	0.150	0 40	0.20	2 00
542	0.15	0.15	0.175	0.175	0.35	0.20	2.00
543	0.15	0.15	0.200	0.200	0.30	0.20	2.00
544	0.20	0.20	0.050	0.050	0.50	0.20	2.00
545	0.20	0.20	0.075	0.075	0.45	0.20	2.00
546	0.20	0.20	0.100	0.100	0.40	0.20	2.00
547	0.20	0.20	0.125	0.125	0.35	0.20	2.00
548	0.20	0.20	0.150	0.150	0.30	0.20	2.00

In these three series, the fluctuating replacing oxides, MgO and ZnO, each advanced through the series by .025 equivalent, the sum of which equals 0.05 equivalent of PbO replaced.

Sixth. We constructed a "quintuple replacement" series, or one in which the PbO was replaced by all the oxides we were experimenting with, simultaneously. In this series, K_2O CaO were constant throughout the series, the other oxides, MgO, ZnO, BaO each increasing by 0.025 equivalent.

QUINTUPLE REPLACEMENT SERIES.

Glaze No.	Molecular Formulae of Glazes.							
	K_2O	CaO	MgO	ZnO	BaO	PbO	Al_2O_3	SiO_2
549	0.20	0.20	0.025	0.025	0.025	0.525	0.20	2.00
550	0.20	0.20	0.050	0 050	0.050	0.450	0.20	2.00
551	0 20	0.20	0.075	0.075	0.075	0.375	0.20	2.00
552	0.20	0.20	0.100	0.100	0.100	0.300	0.20	2.00
553	0.20	0.20	0.125	0.125	0.125	0.225	0.20	2.00

In this series the equivalent PbO decreased by 0.075 equivalent throughout the series, 0.075 being the sum of the fluctuating replacing oxides.

THE MANUFACTURE OF THESE GLAZES.

First. We made up five hundred grams of each glaze throughout these experiments.

Second. We ground the extremes of each series, and blended the intermediates from these extremes. Each extreme was ground for one hour in a ballmill, and as all intermediate glazes were made by mixing the extremes, all glazes were equally ground.

The amount of each extreme which was needed was found in the following manner. For example of this we will refer back to series No. 25 which has been already given in the single replacement table.

We want 500 grams of each of these glazes; therefore, the combined weight of the series will be 500 grams $\times$ 5 = 2500 grams + 500 grams for loss in screening and blending = 3000 grams of stock required. 3000 grams $\div$ 2 = 1500 grams, which is the number of grams of Nos. 300 and 304 glazes which must be ground to produce this series

The calculation of the batch weights of the extremes of each series was done as follows, using glaze No. 304 as an example :

GLAZE 304.

Glaze	0.20 K_2O	0.8 PbO	0.20 Al_2O_3	2.00 SiO_2
0.2 Eqv. Spar	0.20 K_2O		0.20 Al_2O_3	1.20 SiO_2
Difference	×	0.8 PbO	×	0.8 SiO_2
0.8 Eqv. White Lead	×	0.8 PbO	×	
Difference	×	×	×	0.8 SiO_2
0.8 Eqv. Flint	×	×	×	0.8 SiO_2

Calculating the parts of each ingredient to be used:

0.20 Eqv. Spar × 556 = 111.2 × 4.102 = 456.14 grams Spar.
0.80 Eqv. W. Lead × 258 = 206.4 × 4.102 = 846.65 grams White Lead.
0.80 Eqv. Flint × 60 = 48.0 × 4.102 = 196.90 grams Flint.
Total parts in Glaze = 365.6 1499.69 grams No. 304 Glaze

We want 1500 drams of this glaze, therefore 1500 g÷365.6 equals 4.102, the factor by which we must multiply the parts of each ingredient in the glaze to get the batch weights for 1500 drams.

The extremes were weighed out, and ground the required time, in small porcelain ball mills, after which the material was screened through a 150 mesh screen, the slip was then let stand until the solid matter settled, after which the supernatant water was syphoned off. After the water had been syphoned off, and the mass thus concentrated, we mixed each thoroughly and made a moisture test to determine the percentage of solid matter in each slip. For example of determining the percent of solid matter in a slip, and how we used the same in blending each series of glazes, we will take the extremes of series 25, glazes 300-304, from which were blended glazes 301, 302 and 303.

To determine the solid matter in glazes 300 and 304, we mixed each slip thoroughly, and took a sample of each in a dish of known weight.

	Glaze 300	Glaze 304
Weight of dish and slip.....	199.0 grams	195.0 grams
Weight of dish	78.2 grams	74.9 grams
Weight of slip.....	120.8 grams	120.1 grams

We then dried on a steam bath till perfectly dry and re-weighed.

	Glaze 300	Glaze 304
Weight of dish and dry glaze.....	116.1 grams	115.7 grams
Weight of dish	78.2 grams	74.9 grams
Weight of dry glaze	37.9 grams	40.8 grams
Percent of solids in slips...	$\frac{37.9 \times 100}{120.8} = 31.37\ \%$	$\frac{40.8 \times 100}{120.1} = 33.97\ \%$

The factor by which a given amount of a dry body must be multiplied to get the slip weight of same, is found by dividing one by the percentage of solid matter in the slip, or by dividing the slip weight of a moisture test by the dry weight of the same. The slip of glaze No. 300 is 31.37 per cent. solid matter. By dividing one by 31.37, the per cent. of solid matter in slip, we have $\frac{1.0}{.3137} = 3.187$, or by the second method, the weight of the slip in moisture test 120.8 : 37.9, the dry weight in the moisture test, equals $\frac{120.8}{37.9}$ equals 3.187.

The factor by which a given amount of glaze No. 304 must be multiplied to find the slip weight, produced by the second method of calculation equals $\frac{120.1}{40.8} = 2.94$.

BLENDING TABLE.

No. Glaze	Proportions		Weight of Slip to be Used	
	Glaze 300	Glaze 304	Glaze 300	Glaze 304
300	All	None	500×1.00×3.187 = 1593.5	500×0.00×2.94 = 000.0
301	3	1	500×0.75×3.187 = 1195.1	500×0.25×2.94 = 366.5
302	2	2	500×0.50×3.187 = 796.7	500×0.50×2.94 = 735.0
303	1	3	500×0.25×3.187 = 398.3	500×0.75×2.94 = 1102.5
304	None	All	500×0.00×3.187 = 000.0	500×1.00×2.94 = 1370.0

The glazes were then blended according to the slip weights thus obtained, after which each glaze was placed in an ordinary self-sealing fruit jar, well shaken, sieved and replaced in the jar till used.

THE DIPPING.

The glazes after blending, were left standing for a while, generally until the day before we intended to fire, at which time we dipped the trial pieces. Before dipping, we poured off the supernatant water, shook the glaze well in the receptacle, poured through a 100-mesh screen into the dipping receptacle. This screening of the glaze was done to remove any foreign matter that might have got into the glaze, and to insure that the glaze was thoroughly mixed. After screening, the glaze was brought to a proper consistency by the addition of water. The glaze was dipped about one-sixteenth of an inch thick on each trial piece, which was previously marked in blue stain with the number of the glaze in which it was to be dipped.

The trial pieces used were biscuit floor-tile ½ x 1 x 4 inches, which were of three degrees of hardness and of three colors, a white body which was quite soft, a red body which was of medium hardness and a buff body which was much the hardest being nearly vitrified. These floor tile were furnished by the United States Encaustic Tile Company, of Indianapolis, Indiana.

After drying, the trials were put in well luted setters, each body and series being put in a setter by itself, except

in the series where the number of glazes was more than a setter would hold, in which case a series would be put in two setters, but always keeping the bodies separate. For example, we will take series No. 25, which has five glazes. Into each of these five glazes were dipped three trials of each body, white, red and buff. These were put into setters as follows: Three setters of each white, red and buff body; one setter of each body for the three divisions of the kiln. The setters were now put in such tiers and placed in the kiln in such a position as would place the bodies and glaze series as before described, after which the wicket was closed and the firing begun at once.

THE FIRING OF THE TRIALS.

The trials were burnt under tile-setters similar to those used by the U. S. Encaustic Tile Co., the gaps between setters being well luted. The total length of the burn was approximately 22 hours, which was divided into two periods. The first period was devoted to raising the temperature to cone 04, which covered about twelve hours. The second period was devoted to heat soaking or maturing of the glaze, for which about ten hours was used, during which time the temperature was kept between cones 04-02. To accomplish these conditions as nearly as possible, we used a Chatelier Pyrometer and fired the kiln according to a time temperature curve adopted in advance.

The kiln used in these experiments was a down draft kiln, the dimensions of whose ware-chamber were, height 5 feet 9 inches, length 2 feet 7 inches, width 22 inches. The kiln was divided into three parts or zones, upper, middle and lower. The parts were separated more or less by placing fire brick on edge between the setters of the lower and middle parts, and the middle and upper parts. The fuel used was gas coke and coal, just as little coal as possible being used.

The firing was done as follows: The fire was started with wood and coal, just enough of the latter being used, to get a good grate of live fire, after which the gas coke was used, until the point was reached where it would not burn

fast enough under full draft to give the desired rise in temperature. At this point soft coal was put on the coking plate of the furnace. The volatile matter in the coal was gradually roasted out, and mingled with and consumed part of the oxygen of the excess air coming in through the bed of hot coke. After the volatile matter was all gone from the coal, or in other words after the coal was coked, it was shoved back on the grates, and a new charge of coal was put on the coking plate. The coke thus produced was not sufficient to keep the grates covered, so we used as much gas coke as was needed for this purpose, and only used the coal as fast as was needed to raise the heat.

An ideal time-temperature curve was first plotted as follows: We took a co-ordinate paper and plotted on the horizontal line, one hour to each square; and on the vertical line we let each square equal 100 deg. C. Then we counted off 12 hours on the time line, and up to the temperature of cone 04 on the temperature line which is approximately 1070 deg. C.; where these two lines intersected we made a mark and from zero to this mark we drew a line which represented graphically the progress our temperature was supposed to make for the first 12 hours. From this same point, we also drew a line to the intersection of 22 hours, and the temperature of cone 02, approximately 1110 deg. C., which represented the course our temperature should follow during the heat soaking period.

This curve was used as follows: When the fire was started in the kiln, the time was noted, and this took the place of zero on the time side of the curve; from this we marked on each square the time we would arrive at it. While we were raising the heat and until we got near the melting point of cone 04, we took the temperature reading every half hour, after which we took the temperature reading every fifteen minutes until the end of the burn, and plotted same on the curve sheet, aiming, as you well understand, to have these readings come as near the ideal curve as possible.

If you have never tried to fire a small kiln by one of these curves, you cannot appreciate how difficult it is to

follow the ideal curve. The curve that is plotted from the readings will be more or less of a jagged line, the best that can be done, and especially so, when it comes to the heat-soaking, or holding of the temperature constant between cone 04 and 02.

After the burn was finished, the fire was pulled and put out with water. This was done to eliminate as much as possible the sulphur fumes that escape into the kiln, if the fire is allowed to die out on the grates.

The cooling of the kiln was done as rapidly as possible with an open damper.

After cooling, the kiln was emptied, the setters unpacked and each trial piece marked either T, M or B, meaning top, middle and bottom, the three sections of the kiln.

The trials were then taken to a large table, on which they were placed in position in series, each kind of body by itself and in the same relative position they occupied in the kiln. This arrangement gave us very good opportunity to study the different points of each series of glazes, and also compare the different series.

THE STUDY OF THE GLAZES.

The glazes were studied from four points of view, only one of which I will discuss here.

First. The fusion of the series.

Second. The highest workable replacement of PbO in each series.

Third. The appearance of the glazes, as to color and brightness.

Fourth. As to crazing and shivering.

According to Dr. Seger's notes, we had a right to expect a higher replacement of the PbO by the heavier oxides than the lighter oxides, but this was not the case. For example, BaO, which is next to PbO in weight, was the least promising of all the oxides. In all the combinations in which we used it, the glazes were scummy and worthless; a very good example of this occurs in the quintuple replacement

series in the second glaze in the series, which has the formula:

0.2 K_2O, 0.2 CaO, 0.05 MgO, 0.05 ZnO, 0.05 BaO, 0.45 PbO, 0.2 Al_2O_3, 2.00 SiO_2.

This is not a workable glaze, while we have a glaze of the same composition, excepting the BaO, which is a perfect and workable glaze, as far the combination of oxides is concerned.

No single glaze which BaO was used in any proportion was workable, and they are left out of further consideration on that account.

The following are the formulæ of the highest workable replacements of PbO, in the different combinations we have tried:

HIGHEST SUCCESSFUL SINGLE REPLACEMENTS.

0.15 K_2O	0.85 PbO	0.20 Al_2O_3	2 00 SiO_2	All bodies.
0.15 ZnO	0.85 PbO	" "	" "	Red and white bodies.
0.20 ZnO	0.80 PbO	" "	" "	White bodies.
0.15 MgO	0.85 PbO	" "	" "	White bodies.
0.30 MgO	0.70 PbO	" "	" "	Red and buff bodies.
0.25 CaO	0.75 PbO	" "	" "	White bodies.
0.20 CaO	0.80 PbO	" "	" "	Red and buff bodies.

HIGHEST SUCCESSFUL DOUBLE REPLACEMENTS USING POTASH AND LIME.

0.05 K_2O	0.15 CaO	0.8 PbO	0.2 Al_2O_3	2.00 SiO_2
0.10 K_2O	0.20 CaO	0.70 PbO	" "	" "
0.15 K_2O	0.20 CaO	0.65 PbO	" "	" "
0.20 K_2O	0.20 CaO	0.60 PbO	" "	" "

HIGHEST SUCCESSFUL TRIPLE REPLACEMENTS.

0.1 K_2O	0.1 CaO	0.15 MgO	0.65 PbO	0.2 Al_2O_3	2.00 SiO_2
0.2 K_2O	0.2 CaO	0.20 ZnO	0.40 PbO	" "	" "

HIGHEST SUCCESSFUL QUADRUPLE REPLACEMENTS.

0.1 K_2O	0.1 CaO	0.15 MgO	0.15 ZnO	0.50 PbO	0.2 Al_2O_3	2.00 SiO_2
0.15 K_2O	0.15 CaO	0.15 MgO	0.15 ZnO	0.40 PbO	" "	" "
0.2 K_2O	0.20 CaO	0.15 MgO	0.15 ZnO	0.30 PbO	" "	" "

A study of these results will show that the workable limit of replacement of PbO by the different oxides and combinations of oxides used, with the exception of oxide of barium, lies between 0.15 and 0.2 equivalent of each oxide. Another thing developed from the study of these glazes is, that the more oxides used together in replacing PbO, the less lead is required, and the clearer, brighter and smoother the glaze becomes. This is especially noticeable in its effect on the color of the red body. The body does not seem to get darker in color under a quadruple or triple replacement glaze as it does under a single or double replacement glaze.

It will be observed that in these tests, that the oxide of magnesium showed a fluxing power quite different from that which Seger's law led us to expect. We are there told that MgO is the most refractory of the fluxes and barium the least so, next to lead. Whatever the cause may be, we find that BaO is not of any value in these raw glazes in any proportion, while MgO, which we expected to find well nigh useless, gives a higher single replacement than any other oxide and works well in every combination in which it is placed.

THE NEW GLAZE FLUX.

One year later, in discussing the subject of this work, the following idea was suggested by Professor Orton. In Ohio we have an abundance of dolomitic limestones, that are nearly pure dolomites. These stones are burned, slaked, dried and bolted and the product used as a whitening for mortar, and also used in the manufacture of prepared plaster. If this material is constant in composition, and does not contain iron enough to color a glaze, why not use it as a glaze flux in the place of English whiting?

The above tests show that MgO is a valuable flux in itself and will replace lead as well as CaO, and also, they show

that the more fluxes are present at once, the lower the lead may be brought without losing quality in the resultant glaze. If this is the case, we ought to get an additional advantage from the use of a dolomite over a pure lime like the English chalk, in the fact of its higher substituting power in a lead glaze. Also there is a heavy advantage in price, as the powdered Ohio lime can be had for $7.50 per ton against about $20.00 for English whiting.

I took this suggestion up, and have made the following investigation of the possibilities in this direction.

On request, the following firms sent samples of their products: The Seneca White Lime Company, Fostoria, Ohio. The Buckeye Lime Company of Toledo, Ohio.

The analysis of the material used was as follows:

SiO_2	030%
Fe_2O_3	Trace
Al_2O_3	Trace
$Ca(OH)_2$	56.59%
$Mg(OH)_2$	41.59%
	98.28%

I am indebted to Mr. S. V. Peppel, B. Sc., Laboratory Assistant in the Ceramic Department for 1900-01, for the analysis. The deficit in this analysis is thought to be due to CO_2 which the hydroxides readily take up from the air, and it was not thought to be necessary to determine the same at this time.

Transforming the percentage composition of this analysis into the molecular formula, we have:

$$\left.\begin{array}{l} 0.51\ Ca(OH)_2 \\ 0.49\ Mg(OH)_2 \end{array}\right\} 0.003\ SiO_2$$

$$1.00\ RO$$

The SiO_2 was not considered in calculating the batch weights, its proportion being so small.

To investigate the advisability of using this new material in glaze combinations, in place of English whiting, I decided to make nine series of glazes, parallel to the series described heretofore, using this combined hydrate of calcium and magnesium in each, equivalent for equivalent, in place of English whiting.

The following is an example of how this new material was used in place of English whiting.

DOUBLE REPLACEMENT, USING ENGLISH WHITING.

K_2O	CaO	PbO	Al_2O_3	SiO_2
0.15	0.00	0.85	0.20	2.00
0.15	0.05	0.80	"	"
0.15	0.10	0.75	"	"
0.15	0.15	0.70	"	"
0.15	0.20	0.65	"	"
0.15	0.25	0.60	"	"
0.15	0.30	0.55	"	"

DOUBLE REPLACEMENT, USING OHIO DOLOMITE.

K_2O	Dolomite.	PbO	Al_2O_3	SiO_2	Equivalents of CaO and MgO Introduced in the Dolomite	
					CaO	MgO
0.15	0.00	0.85	0.20	2.00	0.00	0.000
0.15	0.05	0.80	"	"	0.026	0.024
0.15	0.10	0.75	"	"	0.051	0.049
0.15	0.15	0.70	"	"	0.077	0.073
0.15	0.20	0.65	"	"	0.103	0.097
0.15	0.25	0.60	"	"	0.129	0.121
0.15	0.30	0.55	"	"	0.154	0.146

The following are the extremes of each series in which dolomite was used:

SINGLE REPLACEMENT SERIES.

Dolomite	PbO	Al_2O_3	SiO_2	Dolomite Equals	
				CaO	MgO
0.00	1.00	0.20	2.00	0.000	0.000
0.60	0.40	"	"	0.306	0.294

DOUBLE REPLACEMENT SERIES.

K_2O	Dolomite	PbO	Al_2O_3	SiO_2	Dolomite Equals	
					CaO	MgO
0.10	0.00	0.90	0.20	2.00	0.000	0.000
0.10	0.35	0.55	"	"	0.180	0.170
0.15	0.00	0.85	"	"	0.000	0.000
0.15	0.30	0.55	"	"	0.154	0.146
0.20	0.00	0.80	"	"	0.000	0.000
0.20	0.30	0.50	"	"	0.154	0.146

TRIPLE REPLACEMENT SERIES.

K_2O	Dolomite	MgO	PbO	Al_2O_3	SiO_2	Dolomite Equals	
						CaO	MgO
0.10	0.10	0.00	0.80	0.20	2.00	0.051	0.049
0.10	0.10	0.30	0.50	0.20	2.00	0.051	0.049
K_2O	Dolomite	ZnO	PbO	Al_2O_3	SiO_2	CaO	MgO
0.20	0.20	0.00	0.60	0.20	2.00	0.103	0.097
0.20	0.20	0.20	0.40	0.20	2.00	0.103	0.097

QUADRUPLE REPLACEMENT SERIES.

K_2O	Dolomite	MgO	ZnO	PbO	Al_2O_3	SiO_2	Dolomite Equals	
							CaO	MgO
0.10	0.10	0.00	0.00	0.80	0.20	2.00	0.051	0.049
0.10	0.10	0.20	0.20	0.40	0.20	2.00	0.051	0.049
0.15	0.15	0.05	0.05	0.60	0.20	2.00	0.077	0.073
0.15	0.15	0.20	0.20	0.30	0.20	2.00	0.077	0.073

The glazes were manufactured the same as the glazes in which English whiting was used, except the burning, which was prolonged ten hours longer than the burn of the glazes in which English whiting was used. However cone 02 in the bottom of the kiln was only started, so that while the time was longer, the temperature was not in excess. Con-

vention time was too near to make another burn possible. The glazes in which the dolomite was used, should be burnt again, and to make the comparison a better one, the glazes in which English whiting was used should be burnt at the same time.

THE COMPARISON OF THESE TWO SETS OF GLAZES.

SINGLE REPLACEMENT SERIES.

First. On the White body, the dolomite glazes show the best fusion. On the red and buff bodies, the lime glazes show the best fusion.

Second. 0.15 equivalents PbO replaced is the limit for both, on all bodies.

Third. Both give clear bright glazes.

Fourth. No crazing or shivering in either.

DOUBLE REPLACEMENT SERIES.

There are two series of each of these that are comparable, and a third series which was made, using dolomite, which is not comparable with the equivalent series, in which English whiting was used. The hard extreme of this dolomite series was what it should be, but the soft extreme, which should have developed most easily was just barely vitrified. This must have been due to some error in weighing.

In the two series, whose composition is as follows:

0.10 K_2O, Dolomite and PbO fluctuating,
and 0.10 K_2O, Whiting and PbO fluctuating,

the following facts were observed:

First. The fusion is better to the upper limit in the whiting series; the fusion of the dolomite glazes takes a sudden drop just after passing the working limit and seems to fall away faster in fluxing power.

Second. On the white body, 0.25 equivalents PbO were replaced in both series. On the red and buff bodies 0.3 equivalents PbO were replaced in both series. On all bodies,

the replacement of the PbO is in favor of the series containing dolomite.

Third. Both give clear bright glazes.

Fourth. The buff body of the English whiting series is badly crazed.

In the two series, the composition of which is as follows:

0.20 K_2O, Dolomite and PbO fluctuating,
and 0.20 K_2O, Whiting and PbO fluctuating,

the following facts were observed:

First. The English whiting series shows the best fusion; being well fused to the upper limit, while the last glaze in the dolomite series is not very well fused.

Second. 0.45 equivalents of PbO is replaced on all bodies in the English whiting series. In the dolomite series, 0.40 PbO is successfully replaced.

Third. Both series give clear, bright glazes.

Fourth. The buff body English whiting series is badly crazed, while the red and white body of the same series, and buff body of the dolomite series are only slightly crazed.

TRIPLE REPLACEMENT SERIES.

There are two of these, one for MgO and one for ZnO. Beginning with the K_2O, CaO and MgO series, whose composition was 0.1 K_2O, 0.1 dolomite, MgO and PbO fluctuating, and 0.1 K_2O, 0.1 CaO from English whiting, MgO and PbO fluctuating, the following facts were observed:

First. The English whiting series is well fused to the upper limit. The dolomite series is well fused to the middle of the series on the white body, the upper numbers being drawn up into glassy beads. On the buff and red bodies, all except the last numbers are well fused.

Second. The English whiting series replaces 0.35 PbO, white and red bodies, and 0.4 PbO on buff body. The dolomite series replaces 0.35 equivalent PbO on all the bodies.

Third. Both series give clear bright glazes.

Fourth. English whiting series, two glazes crazed on the buff body.

Taking next the K_2O, CaO, ZnO series, whose composition was 0.2 K_2O, 0.2 dolomite, ZnO, PbO fluctuating and 0.2 K_2O 0.2 CaO from English whiting, ZnO, PbO fluctuating, we observed:

First. The English whiting series is perfectly fused to the upper limit. The Dolomite series well fused to the upper limit, but dry spots show on the white body, and slight undissolved matter on red and buff bodies at the upper end.

Second. The English whiting series is workable to the upper limit, in which 0.6 PbO is replaced. The dolomite series replaces 0.55 equivalents PbO, and with very little rise in the temperature, would replace 0.6 PbO.

Third. Both give clear bright glazes, with the English whiting series a little better on the white body, and the dolomitic series much the better on the red body.

Fourth. The buff body in both series is crazed, but the English whiting series is the worst.

QUADRUPLE REPLACEMENT SERIES.

There are two of these series for both English whiting and dolomitic stone. The first comparison falls with series whose composition was 0.1 K_2O, 0.1 Dolomite, MgO-ZnO, and PbO fluctuating, and 0.1 K_2O, 0.1 CaO from English whiting, MgO, ZnO and PbO fluctuating. The following results were observed:

First. The fusion is the best in the English whiting series.

Second. The English whiting series replaces 0.5 PbO in all bodies. The dolomite series replaces 0.45 PbO on red and buff bodies and 0.40 PbO in white body.

Third. Both give clear bright glazes, but this is slightly in favor of the dolomite series on the buff body, and decidedly so on the red body.

Fourth. Two members of each crazed on the buff body.

In the next two series, the K_2O was increased to 0.15, and the series became

0.15 K_2O, 0.15 Dolomite, MgO, ZnO, and PbO, fluctuating,
0.15 K_2O, 0.15 CaO from the English whiting, MgO, ZnO, and PbO fluctuating.

First. The English whiting series is well fused to the upper limit. The three upper members of the dolomite series are fused and smooth but are milky from undissolved matter.

Second. The English whiting series replaces on the white body 0.55 PbO, and 0.60 PbO on the red and buff bodies. The dolomite series replaces on the white body 0.45 PbO and 0.55 on the red and buff bodies.

Third. Both give good, clear, bright glazes, but the dolomite series has decidedly the advantage in color on the red body.

Fourth. No crazing.

To see if the trace of iron in the dolomite would affect the color of the glazes, I made three PbO-CaO glazes, introducing same from chemically pure reagents; the PbO being taken from PbO_2, the CaO from $CaCO_3$. The glazes had the formulæ:

0.1 CaO	0.1 PbO	0.2 Al_2O_3	2.00 SiO_2
0.2 "	0.9 "	"	"
	0.8 "	"	"

By comparing with this series, the corresponding glazes made from dolomite, they were found to be colorless; that is, the trace of iron in the dolomite is too minute to color a glaze in which the stone is used.

This series was burnt at the same time that the dolomite glazes were, and can be fairly compared with them as to the workable replacement of PbO.

I have said that we thought we could use a higher equivalent of this new material than of whiting, but from the comparisons of all of the foregoing series of glazes, it was found that in all except two instances, the dolomite replacement was lower than the whiting. I also stated that

cone 02 in the bottom of the kiln was not brought down when the dolomite glazes were burnt, but cone 02 was brought down when the whiting series was burnt; for this reason, the comparison of the glazes is not a fair one, and the dolomite is placed at a disadvantage.

However, a comparison of the series in single replacement of dolomite and PbO was made, and this series made from C. P. reagents is a just one, as they were burnt together. The results show what was expected of the dolomite, and what the other tests failed to disclose.

Comparing these two last series, *i. e.*, the C. P. PbO-CaO series, and the dolomite-PbO series, we find:

First. The dolomite series shows good fusion up to 0.25 equivalent dolomite, while 0.1 CaO from C. P. $CaCO_3$, is not fully fused.

Second. The workable replacement of PbO by dolomite is 0.15 PbO, while 0.1 PbO replaced by CaO from C. P. $CaCO_3$ is not workable.

Third. The color and brightness is about equal.

CONCLUSIONS.

From the nine foregoing series of glazes, comprising a very large number of individual trials, I find the following:

First. That dolomite has been successfully used in place of whiting in all the various glazes here tested.

Second. That in many of the glazes made, the dolomite glaze was a better glaze in color, beauty and lack of crazing than the corresponding whiting glaze.

Third. That an unfortunate accident in the kiln management prevented a strict comparison of the two series, and that the disadvantage was against the dolomite glazes.

Fourth. That in spite of this, they are a very close second to the whiting glaze throughout, in point of fusibility and replacing power, and in occasional instances surpass them.

Fifth. That in the only absolutely fair comparison reached, the dolomite glazes clearly surpassed the corresponding whiting glazes in every respect.

Sixth. That the investigation by Mr. Williams and myself pointed to this conclusion, before the dolomite was under consideration.

While I regret that my work was not more conclusive and freer from defects, still I think I can safely say that in the dolomitic limestones of Ohio, we have a very valuable material for glaze manufacture, which will more than replace English whiting, pound for pound, and which will produce glazes of better color and wearing power, and at a marked saving in cost.

The only question I have in mind is whether this material can be obtained in quantity perfectly uniform, from time to time. Nothing but systematic series of analyses will tell this point, but the producers of the lime surely ought to be willing to bear the cost of proving the uniformity of their supply.

DISCUSSION.

The Chair: I am rather surprised at the action of the magnesia in the dolomite, because I have tried magnesium glazes and always found them off-color.

Mr. Cannan: Here are two glazes, one with one-tenth magnesia and the other with one-tenth lime: here is the same thing in dolomite, the same equivalent of lead replaced in each. (Exhibiting samples.)

The Chair: The dolomite is not as good as either of the others?—I speak from this distance, not having examined them closely.

Mr. Cannan: We were burning the tile in a small kiln. Those glazes are workable, though they have a little unfused material along the end, caused sometimes by having them near the flash of the kiln; sometimes due to sulphur scum. The end of the tile which is away from the wall of the kiln does not have it on. But sometimes we find it on the end next to the bag wall, and we attribute it to sulphur.

Mr. Ernest Mayer: You place barium way down on the scale?

Mr. Cannan: We have to do it. We expected to get a high replacement from barium. I have no sample with me

to show you our results on barium. We were burning with coal, and found the glaze with 0.05 BaO had little specks all over it, which we attribute to sulphur. We tried it then with charcoal, which certainly is devoid of sulphur, to see if we could do away with it, but did not succeed.

Mr. Mayer: The reason I ask that question is that I think we should go very carefully in making a statement contrary to Professor Seger. I had occasion to use barium once, and came to the same conclusion you have; and I then asked for information as to the truth of placing barium next in the scale of fusibility to lead. I was given the answer that I would find it would come close to the lead, providing we frit the barium before putting it in the mixture. So, because you are using a raw glaze, it is not proper to put it so low down in the scale. I think you will find it a very soft flux, considering the conditions.

In making these experiments you have got here (taking up a white tile) a very open body, and here, (taking up a buff tile) practically a vitrified body, I suppose. Now, you speak of putting the glaze on to the thickness of a sixteenth of an inch. With all those different degrees of vitrification in all these experiments comes the question of crazing, and if you have no other system for getting a certain amount of glaze on these bodies than dipping, do you think it is a fair comparison as to crazing?

Mr. Cannan: I made the question of crazing least important, even down to the fourth place. What I was after, was the most fusible mixture. My idea was to find which material would replace the most lead.

Mr. Mayer: I don't care how good a glaze you have, if you get it on a white body, it might be condemned because it would craze. I was going to suggest to you this: In the manufacture of white ware, we use two kinds of glazes, thick and thin, to catch all kinds of ware. Take a hydrometer, which the dipper watches every few hours, which reads from nought to two thousandths of a section. He can keep that set at 28 to 32 ounces, and keep a very even coat of glaze on it. I mention this, because I do not hear anything of the thickness of glazing in regard to crazing.

Mr. Henry Griffin: Regarding the thickness of the glaze coat, you meant a sixteenth of an inch before it was burnt?

Mr. Cannan: Yes. The idea was to get it as near as the eye would tell us, and sometimes we would use a spatula and cut down through it and try to get it as near the same thickness as we could. At first it would be very thick, and the buff would be the first body dipped. Then we would thin it down a little and dip the red body and lastly thinner still for the white. You understand we are not experts, for it takes a long time to learn to do dipping skillfully. We were not working necessarily for a workable glaze on those particular bodies; we were merely trying to determine the highest possible replacement of lead by the various oxides and latterly by the dolomite. Concerning the barium, my paper starts out with saying we were dealing with *raw lead glazes.* You can tell me that Seger says barium will make a glaze *if it is fritted.* I don't propose to put myself up against Dr. Seger, but I have results on the raw lead glazes with barium which we can't question, nor could Dr. Seger question them.

Mr. Mayer: I was surprised in the same way to find barium very much harder, in fact nowhere near lead at all; and I wrote to our friend Langenbeck of Zanesville for an explanation of the fact that barium was put down as such a soft flux, when I found it so hard. His answer was that barium is a soft flux conditionally; that if I knew enough to read Seger, I would find it must be fritted. After fritting, the barium is soft, just as he said.

Mr. Griffin: Did you find in using barium, a tendency to granulation?

Mr. Mayer: I did.

Mr. Griffin: I found by combining fluorspar with the barium, it seemed to take all that tendency away from it, and I got a good smooth barium glaze.

Mr. Mayer: I think we are all very much obliged to Mr. Cannan for his paper, which is a very valuable one and reflects great credit on him.

The Chair : In regard to dipping, if you will soak all the tile to saturation, and use the glaze very thick, as I did last year, you will get the glaze coating even.

Mr. Herbert A. Wheeler: Do you know whether that dolomite comes from a thick persistent bed ? That analysis is remarkable, and the deposit must be of great value, outside of the field of ceramics.

Professor Edward Orton, Jr. : I do not know anything about the persistency or regularity of the dolomite furnished by the company on whose sample Mr. Cannan was working. When I first suggested this problem to him, I agreed to give him help to the extent of writing to certain manufacturers and get samples of their output for him.

I have been too busy to keep track of what Mr. Cannan has been doing since, and am somewhat surprised at the results he has gotten, as I heard them this afternoon. I assure you I have never heard them before, and I expected rather different results from what he has gotten. I expected a more decided benefit from the use of the dolomite. Regarding the dolomite, there are vast quantities of such dolomitic limestone in Ohio. The half of the state is pretty well overlain with limestone strata, and much of it is nearly pure dolomite. I will prepare and append to this article a note, showing what the resources of Ohio are in the matter of dolomitic limestone. In applying for samples for this use, I did not give a hint as to what it was wanted for, and we got what they are sending out to the trade. As Mr. Cannan says, it would take a series of analyses to decide whether we could get it uniform enough to use for a glaze. We do know that English whiting is pretty expensive, and if we can find this on the market much cheaper, and as Mr. Cannan has found, even capable of pound for pound substitution, I believe it is something of a discovery. But when he gets to the bottom of it, I think it will be found capable of even a larger substitution than pound for pound. Mr. Cannan has shown in some of this work that there was some little advantage in in favor of dolomite; but not nearly what he or I expected.

In regard to what Mr. Mayer said about barium, Mr. Cannan prefaced his paper by stating that he was dealing

with *raw* lead glazes. If I can read the German text correctly, where Dr. Seger makes the statement as to the order of fusibility, he makes no qualification as to how it must be prepared. The original statement of Seger is very misleading on that point, for reading that article alone, no one could possibly imagine that fritting was a necessary condition to the truth of the statement.

SUBSEQUENT NOTE, PREPARED BY PROFESSOR EDWARD ORTON, JR.

The question raised in Mr. Cannan's paper, and in the discussion which it aroused, as to the quantity and availability of the limestone or dolomite which would meet the requirements of the pottery industry, has proven unexpectedly easy of demonstration. The essentials which stone must have are (1st) low percentage of iron, (2d) constancy of composition. In the following table are grouped a number of analyses of Ohio dolomites, together with their location. The data were obtained from Vol. VI., Ohio Geol. Survey, 1888:

No.	Name of Producer or Descriptive Matter.	Locality.	Composition			
			Iron and Alumina	Siliceous Matter	Carbonate of Lime	Carb. of Magnesia
1	Middle Bed.....	Springfield, Clark Co.	1.80	1.30	55.40	41.48
2	Upper Layers.....	" "	2.70	1.40	53.90	41.90
3	Average	" "	0.56	0.65	54.13	44.37
4	Geo. H. Frey, Middle....	" "	0.20	0.10	54.70	44.98
5	" Upper....	" "	1.00	1.50	54.70	42.37
6	" Average..	" "	0.39	1.55	53.88	43.79
7	Moore & Co	" "	0.42	0.66	53.51	44.96
8	Sintz & Co	" "	0.37	0.68	53.92	44.99
9	John McCarty.....	" "	0.39	0.59	53.77	44.76
10	Holcomb Lime.....	" "	0.54	1.37	53.44	44.49
11	" Building Stone	" "	0.96	7.37	50.13	40.64
12	D. S. Ervin..	Cedarville, Greene Co.	0.57	0.58	53.95	44.62
13	" "	" "	0.53	0.88	53.90	44.58
14	Irvin & Co., Selected ...	Yellow Springs, "	2.00	0.40	54.75	42.23
15	" Average....	" "	0.51	0.72	53.98	44.56
16	" Build'g Stone	" "	0.76	5.65	50.95	42.64
17	" "Blue" Stone	" "	1.40	5.40	51.10	41.12
18	B. Wilson, No. 1.....	Sec. 17 Wayne Twp. Montgomery Co....	0.83	0.80	54.80	43.69
19	" No. 2.....	Sec. 17 Wayne Twp. Montgomery Co....	0.47	0.39	54.47	44.73

No.	Name of Producer or Descriptive Matter.	Locality.	Composition.			
			Iron and Alumina	Siliceous Matter	Carbonate of Lime	Carb. of Magnesia
20	B. Wilson, Rejected Stone	Sec. 17 Wayne Twp, Montgomery Co....	1.13	4.00	51.97	42.07
21	Jos. Rhule	Covington, Miami Co.	0.40	0.85	54.14	44.90
22	Thomas Corey	Brant, Miami Co.....	0 40	0.32	54.37	44.01
23	James Dwyer, Upper....	New Paris, Preble Co.	0.37	0.61	61.33	37.68
24	" Lower...	" "	0.38	1.23	67.51	30.84
25	T. J Smith, Lower.....	" "	0.56	2.21	67.00	29.26
26	C. Disher, Selected.....	Lewisburg, "	0.60	0.60	55.20	43.28
27	" Average.....	" "	0.45	0.89	54.58	43.93
28	Wm. Turner.....	" "	0.94	1.31	52.70	43.96
29	....	Sidney, Shelby Co....	1.60	.tr	55.00	42.92
30	Bierly's Quarry.....	Greenville, Darke Co.	4.60*	....	44.69	50.11
31	Gard's Quarry.....	" "	2.20*	....	51.30	45.72
32	Northrop's Quarry.....	New Madison, "	2.70*	....	51.70	45.26
33	Newman's Caprock.....	Genoa, Ottawa Co....	0.23	0.04	55.97	44.27
34	Newman's Bottom Rock.	" "	0.51	0.22	53.04	46.01
35	Wyman & Gregg	" "	0.16	0.23	54.30	45.14
36	Habbeler Quarry.....	" "	0.42	0.28	55.59	43.67
37	Holt's Main Rock.....	" "	0.44	0.24	54.61	45.05
38	Kingham's Quarry... ..	Rocky Ridge, "	0.29	0.87	54.10	44.27
39	....	Limestone, "	0.25	0.29	54.12	44.79
40	H. Rancamp & Co.....	Woodv'le Sand'ky Co.	0.39	0.31	53.50	45.79
41	Noble & Co.....	Fremont, "	0.27	0.74	54.06	45.20
42	Gottron Bros.....	" "	0.43	0.35	55.65	42.29
43	.....	Fostoria, Seneca Co..	0.28	0.42	56.41	41.98
44	Surface Rock.....	Bowling Gr., Wood Co	0.40	1.53	53.98	43.25
45	Drillings 210 feet deep...	" "	....	7.76	49.19	40.31
46	Drillings 260 feet deep...	" "	....	0.97	51.62	44.08
47	Wyman, Gregg & Co....	Rising Sun, "	0.63	0.57	53.21	45.37
48	Wyman, Gregg & Co....	Lucky Station, "	0.35	0.43	54.20	44.44
49	W. S. Thurston.....	Bowling Gr., "	0.40	1.53	53.98	43.25
50	Barnd Quarry	Findlay, Hancock Co.	0.39	1.55	53.88	43.79
51	" Rejected	" "	1.16	8.00	49.62	40.94
52	G. W. Fetters.....	Bath Twp., Allen Co.	0.14	1.18	53.69	45.00
53	Norris & Christian.....	Marion, Marion Co...	0.24	1.60	66.02	33.10

*Iron and Alumina and Silicious Matter.

In the fifty-three analyses given in the above table, there are represented fourteen Counties, in different parts of Ohio, and thirty-seven different quarries or actual locations from which samples were taken. It sixteen cases, more than one sample was taken in one quarry.

The marvellous uniformity of these dolomitic limestones over so large an area is a wonderful thing. In this whole area, representing at least two hundred and fifty miles in distance between extremes, we have the iron and alumina fluctuating only from 2.70 maximum to 0.14 minimum. Only eight cases occur out of fifty, in which the sum of the iron

and alumina exceeds 1.00 per cent. In twenty-seven cases, the iron and alumina falls below one-half per cent.

Similarly, the silica is uniformly low. In thirty-one cases out of fifty, it falls below one per cent. The highest silica met in fifty stones is 2.21 per cent., excepting those which are marked as rejected stone, or building stone.

The lime and magnesia in these stones hovers constantly near the typical ratio 56:44, and in three instances, the magnesia exceeds the dolomite ratio.

In view of the above facts, which can be substantiated by a very large amount of additional data if needed, it seems safe to state that Ohio has available an unlimited quantity of very pure dolomite, whose composition is practically uniform over very large areas, and which can be had in the market at a very low cost. If Mr. Cannan's work is borne out by other investigators, and Dolomite is found to be a good economical substitute for whiting as far as the technical considerations go, it is certain that the commercial considerations as to quantity, distribution, cost, purity and reliability will be equally satisfactory.

THE BIRTH OF ENGLISH PORCELAIN.

BY

CHARLES F. BINNS.

Collectors of old china are not infrequently confronted with the statement that a certain specimen has been in the family for hundreds of years. Such a tale is told in good faith, but an investigation will always reveal some error. In the case of Chinese porcelain, rare pieces are found of which the history really covers centuries, but English porcelain was born in 1744, and European porcelain is not many years older.

During the eighteenth century, a tidal wave of porcelain production broke over Europe. The importation of Chinese wares by the Dutch merchants had awakened an appetite for such articles, and the leading men of light and learning began to speculate as to what they were, and how they were made. All sorts of wild tales were current. An old story by an Italian writer stated that "plaster, eggs and shells of marine locusts" entered into the composition, and that the material used was kept buried for at least eighty years, being prepared by one generation for those who were to come.

All these legends were exploded when Böttcher discovered kaolin in his wig, and Europe awoke to the fact that porcelain was being made in Meissen. Between the years 1709 and 1800, upwards of forty porcelain manufactories were opened in Germany, France, Italy, Spain and England. The majority of these were but short lived and nearly all of them were founded by princes and potentates as pastimes and even as playthings. That some succeeded was due more to good luck than good guidance, and those were the times when the practical potter made money. Kings courted and princes bowed down before him, even if his knowledge was of the slightest.

It is not my purpose to trace the history of European porcelain, but to limit this sketch to English soil, relating certain facts which must prove of interest to Ceramists, and extracting from such records as are available some of the technical information which constituted the stock in trade of the potter of that day.

It was in the year 1744 that two men, one described as a merchant, the other as a painter, took out letters patent for the manufacture of porcelain, at Bow near London. One of the ingredients used was a soft earth "the product of the Cherokee nation in America." A second patent taken out in 1749 states that "animals, fossils and vegetables by calcining, grinding and washing are said to produce an insoluble matter, called virgin earth."

The first Bow porcelain was a fritt body, the second contained forty per cent. of bone ash, the nature of which is only thinly veiled in the "virgin earth" just mentioned.

An analysis by Prof. Church of some fragments of this ware, gave the following result:

Silica	40.0
Alumina	16.0
Lime	24.0
Phosphoric Acid	17.8
Magnesia	0.8
Soda	1.3
Potash	0.6
	100.0

From this we are enabled, with some knowledge of the procedure of the time, to deduce certain probabilities. Cornwall stone had not been discovered, feldspar was unknown. The alkalies would be incorporated as a fritt and the silica would be introduced mainly as sea sand. The body could then be nearly reproduced by making a fritt of

Sand	95 parts
Soda Ash	5 "

and then mixing

Fritt	21 parts
Clay	38 "
Bone Ash	41 "

This is, of course, only a rough calculation, but failing the rational analysis of the clay, which probably even the Cherokee nation, though native born Americans, could not supply, we are unable to obtain a much closer result.

It is interesting to note that the territory owned by the Cherokees comprised the states which now produce the best porcelain clay, viz., Kentucky, Tennessee, Alabama, Georgia and the Carolinas. In this the United States may claim to have been the progenitor of English porcelain, even though the parentage was soon repudiated and forgotten.

The procedure at Bow was repeated, *mutatis mutandis*, at Chelsea, on the other side of the Metropolis, but the history of both factories is most fragmentary. Chelsea was chiefly famed for the magnificent colors used. These were undoubtedly made on the works, and served to impart an individuality to the product which is totally lost sight of in this day of the ubiquitous color merchant.

Chelsea porcelain proves upon analysis to be strikingly similar to that of Bow. The knowledge of the virtues of bone ash had become common property, and this substance has held its own ever since as the characteristic ingredient of English porcelains.

It is interesting to compare the formulae of these wares with that of a typical modern English china.

	K_2O	CaO	Al_2O_3	SiO_2	P_2O_5
Bow.....	0.137	2.737	1.00	4.250	1.090
Chelsea.....	0.230	5.200	1.00	7.120	1.500
Modern.....	0.269	2.540	1.00	3.890	1.670

The formulæ have been calculated, taking alumina as unity, for the sake of comparison, and it should be explained that while the figures of the old porcelain are based upon analysis, those of the modern are calculated from a recipe.

The manufactory at Worcester was founded in 1751, and many of the facts of its early history have been laid before the world. There is a special interest attaching to Wor-

cester, in that this is the only one of the old factories which flourishes today.

The Worcester works had a beginning which was remarkable and unique. Certain politicians were desirous of establishing a handicraft of some kind for the purpose of retaining voters in the city, and maintaining the strength of their party at the polls. The bosses were even then active, it would appear, but their activity proved in this case to be a benefit to others besides themselves,

But political wire pulling will not make porcelain, nor were the politicians anxious to establish this particular industry. It so happened, however, that a Worcester doctor named Wall had been experimenting with the view of imitating Chinese porcelain, and had met with considerable success. The inventor and the politicians met, and the scheme was floated.

Dr. Wall's first porcelain was a fritt body, for it was not until some years afterward that bone ash was introduced at Worcester.

Considerable obscurity surrounds the early mixtures, but one of the recipes purporting to be authentic gives:

Sand	120	As the Fritt
Gypsum	7	
Soda	7	
Alum	7	
Salt	14	
Nitre	40	

and for the body

Fritt	76
Whiting	15
Pipe Clay	10

The formula of this fritt would then be

$$\left.\begin{array}{l} 0.523\ Na_2O \\ 0.390\ K_2O \\ 0.087\ CaO \end{array}\right\} 0.01\ Al_2O_3,\ 8.90\ SiO_2$$

1.000

and the equivalent weight 309.

The "pipe clay" would probably be an average English ball clay, such as that of which the rational analysis is given

on page 38 of our "Manual." The formula of this clay, taking alumina as unity, is

$$0.026\ K_2O,\ Al_2O_3,\ 2.71\ SiO_2,\ 1.94\ H_2O$$

and the equivalent weight 302.

The whiting may fairly assumed to be pure. The formula of the whole body would then work out thus

$$\left.\begin{matrix}3.904\ Na_2O\\2.935\ K_2O\\5.277\ CaO\end{matrix}\right\}1.00\ Al_2O_3,\ 31.43\ SiO_2$$

These figures are startling in their interest. In the first place, it is remarkable that a body with so low a content of clay substance could have been worked at all. Two considerations made this possible — the materials were ground very fine, and the body was worked entirely by hand, often with the assistance of a mucilage.

In the second place, how does it happen that such a blend gained the reputation, as it did, of being as hard as Oriental porcelain? If the formula be recast upon the lines usual for glazes, viz.:

$$\left.\begin{matrix}0.322\ Na_2O\\0.242\ K_2O\\0.436\ CaO\end{matrix}\right\}0.082\ Al_2O_3,\ 2.63\ SiO_2$$

it will be seen that many glazes are being used today which are harder than this porcelain body. Where is the secret? I believe that the answer is to be sought in the middle column. The alumina content is the key.

Some of the members may recollect, that in the experiments which formed the subject of my paper last year, it was shown that even very soft lead glazes became practically infusible when the alumina fell below a certain point. Here are the same conditions. Whether from scarcity of clay, or from the poor quality of the pipe clay, or by intentional action, the alumina has been brought to vanishing point and the mass has undergone devitrification. Thus the porcelain body became hard, far harder than a mixture with more clay, and competes successfully with the ancient product of the Chinese.

There is a remarkable analogy between this early product of Worcester and the recent discoveries of Seger.

He has shown that Japanese porcelain is extremely low in alumina and high in alkali, and upon this discovery the composition of Seger porcelain is based. But Dr. Wall has also analysed these Eastern wares so far as the science of his time would permit, and who shall say but that he too had arrived at the correct proportions between the constituent parts of his mix. We have no data upon which to base an estimate of the heat at which this ware was fired. Test cups of the body itself were the only means of control, and these of course tell nothing.

The glaze was a soft compositlon, containing a good deal of lead and no alumina, that is so far as the formula goes. But the silica was largely introduced as sand, so that it is quite likely a proportion of alumina came with it. The formula is

$$\left.\begin{matrix} 0.101\ Na_2O \\ 0.354\ K_2O \\ 0.545\ PbO \end{matrix}\right\} 2.07\ SiO_2$$

We have no information as to whether the glaze were fritted or not, but from the presence of solublé salts and from the general use of fritts at the time, it is highly probable that this was the case.

In the early days of porcelain manufacture, but little regard was paid to the cost of production. The art was a monopoly, and as long as secrecy could be preserved, the factory could make its own price, provided the work was good. From the beginning, the Worcester potters aimed at high-class goods. Dr. Wall set a high standard in quality, and he was eminently successful in the production of the dainty tea equipage, which appealed to the æsthetic taste of the wealthy.

Reading between the lines of history, however, it is evident that there were causes of dissatisfaction in the Worcester ware. A glance at the formula tells us that the losses must have been excessive. It could scarcely have been possible to burn such a body to translucency at less than a fifty per cent loss, and no manufactory could long stand up against this.

Search was therefore continually being made for some material which should enable them to avoid the expensive

fritting and secure greater stability. Kaolin was unknown in England prior to 1755, and even when it was discovered, it remained a monopoly in the hands of the Plymouth and Bristol factories, but is evident that news of the discovery reached the ears of Dr. Wall, before the substance itself was available, for prospecting tours were undertaken in Cornwall with the view of discovering an independent supply. The result was a find, not of kaolin, but of steatite or soap-stone. The greasy feel of this mineral may have led to the trial being made, but in any case, the steatite was shipped to the factory and used in the formula of the second Worcester body. Here it is:

FRITT.

Sand	10
Calcined Flint	6
Potash	2
Smalt	¼

BODY.

Fritt	5
Flint Glass	5
Crown Glass	2
Hard Soapy Rock	1
Soft Soapy Rock	4½

THE GLAZE.

Double Flint Glass	112
White Lead	85
Foreign China	25
Tin Ashes	4
Potash	4
Smalt	½

I remember once hearing a medical man who had served in Canton, China, giving some account of the Chinese system of medicine. He told of powdered snakes and beetles, of syrup distilled from toads, and other delicacies. His feelings on first learning of these specifics, will now be understood in part by the members of this Society.

The first recipes I had to offer you have been reduced, with no small labor, to intelligible formulæ, but in the present case I weaken. Who shall fathom the mystery of "hard" and "soft soapy rock" of "double flint glass" and of "foreign china"?

That which the recipe fails to do, however, can be done by analysis of the ware itself. Salvetat has given us this, and the formula resolves itself into

$$\left.\begin{matrix}0.258\ CaO\\2.078\ MgO\end{matrix}\right\}1.00\ Al_2O_3,\ 15.20\ SiO_2.$$

At the same time, it must be confessed that recipe and formula do not march together. Crown glass and flint glass, double or single, could not have been used without some trace of lead and alkalies appearing, and potash is actually used in the fritt.

Shortly after the experiment with steatite, the Cornwall supplies of kaolin and granite became available, and Worcester entered upon the production of bone china, to which the factory has ever since adhered.

The quality which rendered Worcester porcelain the superior of its contemporaries and contributed doubtless to the continuous life of the factory was that of workmanship. A piece of old Worcester is rarely or never seen with a slovenly or imperfect finish.

Not only as regards the manufacture but in the decoration this is apparent, and even in the printed pieces, a moment's comparison with the work of either Liverpool or Caughley will at once demonstrate their inferiority.

Notwithstanding the fact that Worcester possessed neither coal, clay, nor skill, the factory paid its way from the first, and the reason is not far to seek. The beginning was on a modest scale, and nothing but the best work was tolerated. There is always a market for good things, and for those who are able to produce them, and are at the same time content to make small beginnings, there must be a productive future.

How far these principles may meet modern ideas remains to be seen, but in the interests of the porcelain production of America, the idea is put forth.

The discovery of Kaolin stone in Cornwall gave rise to the factories at Plymouth, and Bristol, both of which made hard porcelain, but neither of which was destined to a long life.

Feeling that the members of the Society will prefer the

technical to the artistic, I refrain from any comment upon the latter feature of these wares, and offer a series of formulæ comparing the wares, from Plymouth and Bristol with Sevres hard paste and Chinese porcelain.

	K_2O	CaO	Al_2O_3	SiO_2
Bristol	0.0707	0.086	1.00	3.22
Plymouth	0.1160	0.000	1.00	3.75
Sevres	0.1290	0.207	1.00	4.39
Chinese	0.3333	0.020	1.00	4.72

The extraordinary hardness of the English wares will at once be noticed. Prof. Church has called attention to this and remarks with a melancholy interest upon the fact that when his magnificent collection of porcelain was distroyed by fire, the product of these factories withstood the terrific heat while all the other English porcelains were fused to glassy lumps.

I will invite your attention to one more fact which is that the glaze used at Plymouth was evidently composed of Cornwall stone, with the addition of lime. The formula is:

$$\left.\begin{matrix}0.484\ K_2O \cdots \\ 0.516\ CaO \cdots\end{matrix}\right\}\ 0.955\ Al_2O_3,\ 7.64\ SiO_2$$

and, given a sound and undecomposed sample of the granite, this glaze might be well reproduced by 90 parts of stone, and 10 of chalk.

This paper has departed somewhat from the normal lines of our contributions, but there are two reasons for this. The first will be found in the able and, unfortunately, unrecorded address given by our President at Niagara Falls; the second lies in the fact that circumstances have rendered it impossible for me to pursue any original research since the last meeting of the Society.

If it be permissible for me to point the moral of my story, I would sum the matter up in a question.

If it were possible for earnest men in the comparative darkness of the eighteenth century, to establish and perfect

a manufacture which required a high degree of technical skill and artistic taste, how much more should the men of the present day, when technical schools and art institutes abound on every hand, devote their energies to the development of all the possibilities of their art and raise their country, their calling and perhaps themselves to a position recognized by the critical opinion of the world?

DISCUSSION.

Vice President Mayer (occupying the Chair): You will note the fact that before our friends in England could make porcelain, they had to come to America to get material. I would like to ask one thing about the formulæ: what is this unity of Al_2O_3? I merely ask the question, because I am afraid it may make confusion for us.

Professor Binns: In adapting the chemical formula of the body, I have taken the lines adopted for clay, taking alumina as a unity. You will find it so given in the manual. Professor Orton will explain it to us, I am sure.

Professor Orton: From my point of view, in any compound which we attempt to represent by the molecular formulæ, that element should be taken as a unity, which we deem its essential or characteristic constituent. In a glaze, we take the sum of RO elements, or some single RO element as unity, because it is more convenient to judge of the fusibility, etc., from that standpoint. In a clay, the clay substance is the essential feature of the substance, and it seems therefore proper that its base, the alumina, should be taken as unity. If we were to write the formula of a clay, and take the small traces of alkalies and earths present as unity, we would make a very poorly proportioned formula, in which the alumina would run up to four or five molecules and the silica from fifteen to fifty molecules. In a flux, like iron ore, I take F_2O_3 as unity. In bone ash, I think CaO should be unity, etc. In short, the unity should represent the characteristic feature of the substance, whatever it is. In a clay, Professor Binns has rightly taken Al_2O_3 as the basis, for on that basis only can it be easily compared with kaolin, the type clay.

Professor Binns: I want to call attention once more to the remarkably low content of alumina in the original Worcester body. Take the Bow and Chelsea body; we have the unity of alumina as against 4.250 and 7.120 silica; take the Worcester body, we find unity of Alumina as against 31.43 silica—a very low content comparatively of alumina. Why did that body get the reputation of being as hard as Chinese porcelain? It seems that fact is worthy of note by ceramists, because there is a great deal behind it and I would like to have it brought out.

Mr. Stanley G. Burt: I would like to ask Mr. Binns if he has ever noticed the fact, that in certain glaze formulæ, the addition of the flint does not necessarily render that glaze hard? As we reduce the acid, the mixture actually gets harder and more infusible.

Professor Binns: By reduction of flint?

Mr. Burt: Yes. If you take formula of a Seger cone, and gradually reduce the flint in the mixture, you might imagine at first that you are going to arrive at a constantly lower-fusing body. On the other hand, the flint taken from that mixture makes it practically infusible. The excess of the silica present in the Worcester body does not carry with it the sense of hardness to my mind that it does to Mr. Binns, after considering the fact that it is very low in alumina. In fact the silica actually acts as a flux in certain formulæ. I understood that Mr. Binns said the Worcester body was a hard body, because of the high content of silica.

Mr. Binns: No; I said "due to the low content of alumina."

THE USE OF GLAZED CLAY-WARE AS AN ARCHITECTURAL DECORATION IN EXTERIOR VS. INTERIOR WORK.

BY

OTTO HENSEL.

If you expect anything comprehensive, I must disappoint you at the outset. At present I am able to give but a few disconnected observations on the theme, whose immensity I discovered only on trying to write about it. Everything that is excellent in glazes is embraced, and its mastery requires a good deal of concentrated study.

Nevertheless let a person but start in finding shades for decorative effect, and he is at once confronted by the problem with all its mysteries. Merely to duplicate a glaze is simple enough, yet frequently the problem is to vary existing or find new effects. Frequently the experimenter must ask and answer himself, "What feature makes this glaze valuable for that purpose?" However mean his ability, he has to meet the subject as squarely as the greatest master of the craft. It is necessity spurring him on, over again.

The subject divides itself into decoration of the interior, of the exterior walls, and of the roof. Effects must be studied by the laws of color as laid down in good manuals on chromatics and by those governing the art of building as found in works on architecture. Above all these aids, the student must command a share of good taste and of a healthy critical faculty, and cultivate them. Furthermore, he can only hope to make progress by the study of concrete examples, actual architecture surrounding him. Chicago is a fertile field for such work.

You will perhaps tell me that this is an unpractical scheme leading nowhere; that tastes are as numerous as individuals; and that in practical work, one must give what is called for. Still a study of the truths underlying the art will give the student his bearings, and he is then free to put his cleverness to the test in meeting exigencies of the moment and perhaps bringing a new contribution to the art.

Interior effects in the broadest sense may be taken to include all pottery from porcelain to terra-cotta articles. But then articles of the coarser sort, such as majolica vases, are just as effective out of doors, provided they are are placed on a level with the eye or below it, to enliven a corner of the piazza or of the lawn. Interior effects proper refer to mosaic floors, tiling for walls, and ceiling decoration.

The glaze has a distinct purpose here, according to established laws of chromatics and of architecture. The checkered floor emphasizes the spacousness of the room and the walls give character to it in keeping with its intended use; the ceiling seems to be partly independent of the rest, for its decoration, while depending on the wall, diverges from it.

The effects desired appear to be obtained mainly in three ways, underglaze, which may be strengthened or altered by color in the glaze, colored tin enamels, and transparent or colored glazes over relief, where the varying thickness of the overlying glazes produces light and shade. What is desirable here seems to be either a strong metallic reflection especially suitable for back ground, or a great depth of lustre combined with play of colors. The chemist works in a very complicated fashion, applying freely colored engobes, variously colored clays laid side by side, a great deal of fritt for perfecting lustre, specially prepared colors, and the like.

Decorative effects on the exterior walls are becoming very frequent. Our technical periodicals state that we are entering an epoch in architecture showing a distinctly picturesque tendency; that we demand no neutral tints but bold general effects and harmony of color. I do not venture to state to what extent the effects prevailing for the interior can be utilized here. I have seen little in that line. Several

small store-fronts with the narrow strips of walls left beyond the window spaces, laid with richly glazed brick in mosaic pattern, may be classed, I suppose, with what Ruskin styles "commercial art." I saw one church, of broad, tent-like outline and severe style of architecture, of glossy white, though no particularly offensive lustre,—a significant departure. I noted that matt surfaces, flowed smooth for the sake of cleanliness, prevailed. If harmony is brown, as has been stated, I should think it might be used more, provided of course that a suitable attractive glaze is available. Thick and lustrous majolica glazes find a sparing use in outside decoration.

The glazing technique is much simpler. Frequently the chemist can perhaps do without fritts altogether, bringing to aid ground glass and calcium borate, a compound which forms in the raw glaze when quick-lime and boric acid in equivalent amounts are ground with the rest. These glazes are exposed to the changes of temperature the year round and therein lies a difficulty that he who compounds glazes for interior work hardly meets in his channel. Owing to nearness to the observer, the chemist must see that his colors come out clearly and properly in the glaze or englobe, hence some care is necessary in their preparation.

The roof, once decided that the house shall have one, is a separate member in architecture. It does not apologize for being there and can be treated with all the boldness consistent with the plan of the house. Unlike the wall, which may have a dark shady street, or a grove of sombre-looking trees, or an expanse of warm rich nature for background, the roof in our latitudes always has the same background of cold blue. Hence we find suitable either a dark intensely matt surface or warm colors. Thus it suggests massive structure or harmony of color. Owing to its distance from the observer, the coloring must be strong, no half tints. Metallic-looking enamels are out of place here. The gloss of majolica glazes may be employed, but more generally the more matt the effect, the better. It seems to me that of glossy glazes the one that contains the color suspended

without being completely opacified, is much more effective than simple undergaze effects.

Although much can be learned from the more complicated departments in the art and chemistry of glazing, the greatest simplicity of procedure prevails here. The effect will be good even with a blotchy color, provided it be strong.

DISCUSSION.

Mr. W. D. Gates: I suppose that among the many things that tend to make the sad life of the terra-cotta man sadder, and makes the nervous prostration that ultimately awaits him almost welcome, none is more appropriate than this question of exterior decoration with glaze and enamel. We have, of course, to furnish our ware as people want it, and there is a growing demand for buildings with enameled fronts, which we have to meet. One of the things which tends to hasten this nervous prostration is the fact of constant haste or pressure. You potters, in making a cup, know that no man is waiting to drink out of that cup. He has something else to drink out of, while you are working. He can use sea shells, tomato-cans and other articles. The sanitary potter need not endanger his bath tubs because a man is waiting; the man can go and dent the surface of the nearest river, if it is necessary. But when a man makes up his mind to use architectural terra-cotta in his building; than our fun begins in earnest. In the West, we have a feverishness in doing things. There is such a thing as time in some places; they have another day, down South. We haven't got it in Chicago—there is only one day. There is always that everlasting rush. If you get tied up in a contract with a forfeiture of five hundred dollars a day, and if you don't have it completed at such a time, you are "it."

Then the question of color comes in, in such a violently aggressive way. The architect has a scheme; he sees in his mind's eye what he wants, and we show him a little sample. (We can all make good samples—I always prided myself that whatever I sent to the building, I could always send good samples to the architect, and I have a poor opinion of

a man who can't make good samples). The architect must decide from that little sample, as to what it will look like in large masses; then we must give it to him. Last year, one of our customers had a mania for a blue-white building, even though he had to tear down another building to get it. The next man must have a creamy white, with more life in it. Blue and white was too cold.

A dull, matt surface will not keep as clean as a polished surface, and a man will run the risk of reflected light from large surfaces, the unevenness of which will throw down the light in bucketsful. The dull surface will not do this. The troubles we have with our architect friends keep us moving. I remember straining what few nerves I had left to make a twelve-foot fluted column. I succeeded and then the architect wanted a twenty-five foot column. You often wonder at my sad appearance, and drooping figure, and that is the reason.

The Chair: There is one point I have often wished to speak to architects on, and I wish there were twenty here.

They make, up at Alfred, N. Y., a very good grade of roofing tile. The architect's specifications for one of the buildings at Alfred University insisted that the tile be of uniform color. We have such a roof on one of the buildings now, and the pieces of tile are as near alike one another, as sticks of sealing wax; you can't tell one from another. I think that is a mistake.

I recently had occasion to build a chicken house, and I went out to this roofing tile yard, and said that I wanted some waste tiles and was given what I wanted from a cull-pile. I selected what I wanted and put it on, and I have the prettiest little roof in the country. It gives all the shades of clay product, as it is run from the machine; just such variations as we know in a nice red brick building. We all can recall instances of a brick building painted red, with the lines carefully penciled out in white. I can hardly describe the effect it has on me. It is an absurdity. Clay products should not be absolutely uniform: we lose all the play of light and shade in which lies one of the true values of the clay itself.

So I insisted that I would not put on this uniform red tile. I went to the tile-yard once more and said that I wanted those old tile, instead of the uniform ones which the architect had specified. I found a large stock of those tiles of different shades, and got some brown trimmings, and we now have a mottled roof which is as much superior to plain, one-color roof, as gold is superior to lead. I wish that architects would take our experience into consideration.

Mr. H. B. Skeele : I want to say that our experience bears out very fully what Mr. Binns has said regarding the growing demand for variety in color. Formerly, roofing tile was called for of a uniform shade. Now scarcely one architect in ten will call for a uniform shade. They even go so far that the natural variations of shade of the tile coming from the kiln, which is large, is not sufficient to meet the demands. On one occasion we had to make a special mixture to bring in four different shades quite widely apart, for one order, in order to get this variegated effect in the roof, which is, as has been rightly said, so much more desirable.

THE AMERICAN CERAMIC SOCIETY AND ITS RELATIONS TO THE PRACTICAL POTTER.

BY

EDWARD C. STOVER.

In reading this paper I desire to call the attention of the practical potter, particularly the men who compound the bodies and glazes in our various potteries throughout the country, to the ultimate aims of the American Ceramic Society, which are to improve and facilitate the manufacturing of ceramic products in the United States. In order to do this, one of the duties of our society, is to collect and publish literature on the subject. At its meetings, papers are read and discussion follows on the various subjects of importance and interest. The theoretic, scientific and practical sides are all fully represented.

The practical man is apt to think or say "I care little or nothing for theory, and know little, and care less, for science, but confine my attention exclusively to the practical problems of my business."

Of the practical man who professes the above sentiments, have you ever asked the following questions: What is Theory? What is Science? If not, kindly reflect on the following.

A theory is a conclusion or reason drawn from numerous and accurately observed phenomena, or a deduction of the laws which connect cause with effect.

Science is the recorded experiences and discoveries of mankind, or the knowledge of many, collected and methodically digested, and arranged so as to become easily available.

Every practical man is therefore consciously or unconsciously, more or less of a theorist and scientist; to what extent, predetermines and facilitates the results of his practice.

Every up-to-date and ambitious practical potter should become a member, or associate, of the American Ceramic Society, and attend as many of its meetings as possible, to hear papers read touching the perplexing points in his business, and to enter into the discussions that follow each paper. If the particular points upon which he would like to have a little more light, do not come up, it is then his privilege to bring them up and have them discussed. The exchange of ideas with the members of the society is not only a pleasure, but a permanent benefit to all who hear or read the discussions. It is true that some chaff is always with the wheat in this organization as well as in all others but it is an easy matter, for one skilled in the art, to separate them.

The practical potter in whatever branch of the business, who is making some progress, and expects to keep abreast of the times, should keep in touch with this society, as he would find, from time to time, in its papers and discussions extremely helpful suggestions.

Every practicing potter finds at times the desirability, and frequently the necessity, of making changes in his mixtures, and is required often times to substitute new materials for old, and it is in making these changes by the "rule of thumb" or guessing method, that much vexation and trouble arises. Fortunately the laws of nature are such that, by the assistance of scientific knowledge, much of this trouble can be obviated, and the results desired can be readily figured by simple methods, thereby very materially lessening the experimenting necessary to make the proper change. For example, it is frequently desirable, for obvious reasons, to substitute feldspar for Cornwall Stone. With the assistance of the "Manual of Ceramic Calculation" published last year by our society, any practical man can easily learn how to make the above and other substitutions without materially affecting the constituents of a glaze, in which said change is properly figured and made.

If you are using Cornwall Stone in a glaze, and want to change to feldspar, for every 100 lbs. of Cornwall Stone omitted substitute 36.2 lbs. of feldspar, also add 25 lbs.

China clay and 43 lbs. flint. It is necessary to add both the clay and flint to the mixture, for the amount of spar which will give the same quantity of potash as was contained originally in the stone, does not carry with it the required amount of alumina and silica. The feldspar figured in the above example is orthoclase, or potash feldspar, but any feldspar, or, in fact, any of the materials used, can be readily figured.

A casual glance at the progress of the present meeting, or in fact, any of our former programs, should be a sufficient guarantee to any one interested in Ceramics, and particularly to the ambitious practical potter, to awaken an interest in the work of our Society.

The benefits of increased membership are mutual—the more practical men that attend our meetings, the more interesting, and instructive will our discussions become. On the other hand, the theories advanced on the scientific points brought out will be more apt to be put into practical use, and thus the benefits and influence of our Society will be more quickly and surely felt.

The benefits to every American potter of a general raising of the standard of the quality of ceramic products of this country are so obvious that every thoughtful man must realize that an improved quality means increased demand, and as the standard is raised at home, we shut out the competition from abroad. At the present time we are ahead of the rest of the world in the manufacture of sanitary earthenware, including the heavy line of bath tubs, lavatories, etc. Several very creditable lines of genuine porcelain ware are now being successfully made in the United States. It is our hope that the day is not far distant when the very finest, most desirable, and most artistic ceramic products of every class will be made here, and it was with a view to hastening the time when this standard should be reached, that our charter members formed the American Ceramic Society, and it is with this motive that we invite the attention and co-operation of every practical worker and person interested in Ceramic work.

Each member and associate member in good standing receives printed copies of all papers, including the discussion that follows.

DISCUSSION.

The Chair: Gentlemen, you have heard Mr. Stover's paper. Now is the opportunity for the practical potter. There are a number of them here. The American Ceramic Society is on trial; if anyone wants to criticize it, now is his chance. If anything is being done which ought not to be, or anything should be done, which is being neglected, let us hear of it. Mr. Burt has told us that the makers of the Chatelier pyrometer have sent out inquiries to users of the instrument, asking them to say what was wrong with it; they did not want them to praise it, but to tell them what was wrong with it, so it could be made better. That is our position. Tell us what is wrong with this society and those interested will try to remedy the evil.

Mr. William Burgess: I feel, as a practical potter, that I would like to get a knife into my fellow-potters and stir up those on the outside. I never realized until Mr. Stover was reading his paper what I have been missing. Twenty-four years ago I took my B. S. degree at Princeton. A few years later, I took the M. S. degree, and then went into the pottery business and let it all drop. From that time to this, I have used little of the knowledge I gained then, and if a society of this kind had been in existence then, it would have been everything to me; but now I am as rusty as it is possible to be. And I would like to emphasize the invitation Mr. Stover has extended, especially to the young men starting in the pottery business. I advise them by all means to come in and use the theory and science being developed by the members of this society.

Mr. Yates: I can say, Mr. President, being brought up in the old school, that this seems to be living in a new world. The old familiar "bone, blood and body," we don't hear any more. We have got onto "fluxing material," "body" and "base."

The Chair: The fact is, Mr. Yates, we have got down to the "marrow."

Mr. John W. Walker: This is my first appearance here, and I am here at the instigation of Mr. Stover, and my regret

is that I did not know there was such a society before, and have missed what has passed before. I shall become a student of the papers already in the publications of the society. I have enjoyed the meetings, and profited by them, and promise to do my part to the best of my ability as a member.

Mr. Burgess: You inquired for suggestions. All of us have not been educated scientifically and along chemical lines, and I will say that in going over the different formulæ and recipes, instead of using entirely scientific names and terms in the formula, I suggest that you use the common expressions as we know them. For instance, when lime is spoken of, let it be named in the form generally used in pottery. We know what lime is, some of us know what Paris white is, some know other compositions, but we don't all know the chemical term, and I have heard expressions of regret at this meeting that the common names were not used in explanation of the formulæ.

Mr. Stover: I think the point is a good one, and can be carried out without detriment to our literature. I think if we attract the interest of many outsiders, we have got to get down from the H_2O business and say " water." We can put the formulæ in parenthesis. You present that (indicating formula on board) to the practical potter, and he scratches his head and walks away from it.

Mr. Gray: I am glad Mr. Burgess has had the courage to make such a statement. I have been much at sea regarding these chemical formulæ, and I hadn't the hardihood to show my ignorance of it, so I kept very quiet. But I am glad he has been willing to bring it out. I have tried to study it up, but I found the chemical symbols don't always mean the same thing when used by different people.

The Chair: Pardon me, but that is just why we do use them: because they do mean the same thing. When you use the terms China clay and Cornish stone, they don't always mean the same thing.

Mr. Everett Townsend: I don't think the use of chemical formulæ is an obstacle. I have not had any chemistry in school, but just by getting a chemistry and studying it

up, I became quite familiar with the terms; and I now like it expressed this way rather than the other way, because I can get my CaO and my Na_2O, etc., out of different materials.

Mr. William D. Gates: I agree with that. When I went to school, I once knew a man who had neglected to learn his Latin grammar, and when he came to read Cæsar, at the boarding house, it was necessary for everyone else to move out. He locked the door, and proceeded to get his Cæsar by main force. My chemical education, like his Latin grammar, was unfortunately neglected, and I have had to proceed with my investigations by main strength as he did with his Cæsar. But I have endeavored, earnestly endeavored, to try to bring myself to the point of my associates here, to try to bring myself to understand the formulæ as expressed, because I think it is the best way to express them. I earnestly intend to try to bring myself to use them that way, and I think others can do the same.

Professor Edward Orton: I would like to ask these gentlemen who have asked us to write our ideas differently, whether they think we prefer to write in hieroglyphics, or whether they think we adopt this method for the purpose of mystifying them. If so, they are most deeply mistaken. We write these formulæ in this manner, because we can convey an idea that way which we cannot convey in pounds and ounces. I defy anybody to take a book of potter's recipes, of which there are several published, and from a study of the body mixtures and glaze mixtures as given in those books, get the philosophy of body and glaze out of it.

If, on the other hand, you take out and assemble from those various different ingredients, the chemical elements of which they are composed (and you will find for the body four or five elements and for glazes about eight—that's all) you can soon begin to see the law and order beginning to emerge from chaos. You who do not deal in CaO, K_2O, etc., have only got to take fifteen minutes, or an hour, to memorize a few of those symbols and you will then be able to use them right along. A man who learns to estimate the value of a glaze or body from the formula, has acquired a method of mental analysis, which can't be obtained in any other

manner. We don't do this, because we want to make it hard or uncomfortable for others who haven't learned that way, but because the facts cannot be clearly set forth in any other manner. You must come to this method; not expect us to go back to the old way, which has failed in all these decades to bring any orderly understanding out of ceramic processes and results.

Mr. Burgess: I don't want to be misunderstood in regard to this discussion. I did not suggest this because I think the other way is not better; but in reading the papers or in discussions, it seems to me a little side light might be thrown on the terms in use. For instance, in speaking of calcium you might say, one of the most accessible ways of getting it is from Paris white or whiting—merely giving a passing word to that effect. I have heard this criticism passed since I have been here. What has been said has been intelligible to me, but to others it has not, and I merely suggest a little running explanation for those not versed. Of course, the only way to get to a proper analysis, is through the method you have already adopted.

The Chair: I think this discussion will do good. It will make the position of those using the formulæ clear to those who are not, and perhaps raise more sympathy in the minds of those using them for those who are not. It is a matter which is not new to us. We have threshed it out over and over again, and when we published our manual of ceramic calculation, Professor Orton and I had about the nearest approach to a fight we ever had, about it. I had pretty much the idea announced here today, that we ought to go to the level of those who did not understand chemistry and try to lift them up. He took the ground that we must presuppose some knowledge of chemistry, because it is absolutely impossible in a book like that to put it in any other way; and those who did not have it, must refer to some elementary book where they can get a knowledge of the elements of chemistry.

No man can force these things down yours or others' throats; they will live, if at all, on account of their intrinsic value. Mr. Burgess refers to Paris white as the principal

source of lime. We get it from numerous sources, but it is all CaO. It is just because formulae *are simple* that they are used; not because they are complicated. And I counsel those who do not understand them to get right down to it, and learn to understand them, right away. By so doing they will find an enormous accession of power and understanding.

I had a similar experience in my little class at Trenton. I started right in with those formulæ, and Mr. Stover knows that some of them shook their heads; here one dropped out and there another dropped; but some held on; Mr. Townsend, who testified awhile ago, was one that held on, and he is now reaping the advantage of it. What he has done, others may do. As our Secretary says, you had better get right down, and learn them. We could write potash. soda and lime in our formulæ but, *cui bono*? What is to be accomplished? A little study will put you past the need of it. You would be surprised if you would get some of these recipes to which the Secretary alluded, and figure them down to their bones, to find scarcely a pin's point difference between them. They are almost identical.

Mr. Stover: Unfortunately, in this discussion, the fellow you are firing at is not here; and when any of us approaches him, he looks at us with sympathy and says, "No, No," and walks away; and if you give him some of our discussions, it simply confirms his suspicions. I think if you bracket and put in the most common form, the material that is described so the potter who gets hold of that paper can see something he can understand, it will bring him to the meeting; and I believe a practical potter who comes to the meeting, will go home and get an elementary text book and study.

Mr. Yates: I think the remarks of Mr. Stover should be considered by the society. What little I know of the clay business I have acquired from the practical standpoint, and I assure you that the scientific chemist is not the man to draw in the practical man. He has a perfect dread of him, and when he sees him, will say "There's a fellow who is too many for me." If you only draw the men in and get them interested, they will gradually come up to the society. If you discourage them on the start, you will never get them

here. Practice makes perfect, and what is familiar to Professors Binns and Orton is simply Latin to the practical man. When I first started some years ago, I got a school book, and started to study it, and you should have heard the ridicule I got from my wife. But I stuck to it, and now I can read the symbols on the board; but I can't compound formulæ as I would like to. I think Mr. Stover's suggestion should be cherished, and the society will grow. Science in the abstract is a very fine thing, but science applied is better.

Mr. Otto Hensel: It seems to me that a former speaker brought out these points. You will find them already published in good manuals, especially in the German. You find all the points mentioned—how such an oxide has a certain influence and produces certain colors. The practical potter finds these things himself, because he is always experimenting. But it seems to me, with all due respect to the practical man, it is a question of the "Mountain coming to Mohammed" or "Mohammed coming to the Mountain."

This Society published a Manual last year, and after looking through it, I think it is a pretty good book. It covers all the points, it seems to me, that a man must know in getting down to this work. It seems to me if the practical man will make some effort, and see what he can do, he will make progress and master the thing all right.

When I left school, I happened to be in a state where the clay-working industry was more advanced than in my home state, so when I left school I traveled around the country to see what I could pick up in practical points. I came to a place where the superintendent of the works had been a mason, and a contractor; he had been put in his present position because he was a good executive, a good manager. But he was always anxious to learn, and he got me to give him some lessons on these very calculations. After only a few evenings work, he had mastered it so he could work it himself.

Mr. Bloomfield: As I understand it, the American Ceramic Society was formed as a means to an end, that end to elevate the ceramic industry of the United States and the world. If, by putting in parenthesis the terms which will

explain the chemical formulæ, you thereby enable us to bring in to the society the man with no chemical education, but who is practical; what is the use of keeping that out. I know myself the effect will be that the men who read those books, and essays, will acquire the habit of mind, by reading the formulæ opposite the explanation, which will lead them bye and bye to look out for the primers so they can study those things, and in the natural course of events they will become apt scholars, and we will reach the point for which this society was formed. This society was formed to bring in these men, and elevate them, and if you can elevate them by going out of your way to do it, that is the proper thing to do. The higher up in the world a man is, the lower down he can reach to help a fellow man. Otherwise, what is the use of education.

Mr. Mayer: In answer to Mr. Bloomfield, I will say this society is very anxious to impart all the knowledge it can. I speak individually, as possibly I have not the right to speak for the society, and this is only my opinion gathered from discussions I have heard among members generally. I think the idea of the society is that it is willing to impart all the information it can, but we have got to do it in our own way, which we believe is the best way. It is a very easy matter to familiarize yourselves with these formulæ; very easy, if you think the matter over.

I don't think there will be any very radical change by the society in the method of giving out information. We have got to give it out in the methods universally adopted, not only in this, but other countries, and I think the gentlemen present will see the reasonableness of the ground on which we stand; and if those gentlemen who don't understand this, will spend a little time looking matters up in the chemical books, they will not have much trouble in the future.

Professor Orton: One last word. There has been a great deal said here today about the purposes and aims of the society and much stress has been laid on the altruistic spirit, by which it is actuated. But, it would be wrong to let people think the society was formed wholly for the uplifting of the present generation of clayworkers. If we

had only this end in view, it is doubtful if the society could be kept together.

In fact, gentlemen, this society was formed like most others, from motives of distinctly another sort. It was from a desire of educated and trained ceramic workers to get together and discuss freely and in congenial society, the things for which they cannot find sympathetic ears elsewhere; and by exchange of knowledge, to broaden and perfect themselves. When we organized, no such society was in existance in any English speaking country, and I think I may say that its two years of life have very fully realized these objects which I have just enumerated, for all those who have joined it.

I don't mean to say that we had no high-minded unselfish motives in organizing this body. On the contrary, we thought it would be a good thing for the country and for its ceramic industries to have such an organization in it. We thought it would exercise an uplifting influence on clay-workers of all sorts; not directly perhaps, or at once, but in the broad way. Any organization which helps the leaders of an industry to grow perfect in their work, and to enjoy and love their work, helps the rank and file also, in the long run. In my opinion, the way in which we, as an organization, can best advance the interests of the American Ceramic industry, is by diligently laboring to make this society recognized the world over as the most learned and progressive body of men who are working in this field.

How can we attain this result? We meet but seldom and must utilize our minutes, for they swiftly slip away. Shall we spend our time in discussing how to place our facts so simply that people can absorb and assimilate them without working? Who did that for us? Have we not had to labor, hard and patiently, for many years, to get together that which we have? And, has not the struggle which we have gone through been the greatest gain to us, after all? It is because we have struggled and worked and used every opportunity, that we have been able to get such a society into existance. And, it is the struggle which we have been through which makes us able to appreciate such a society.

In my judgment, the greatest good this Society will ever do, will be to set a goal before the American clay worker to which we can inspire him to climb. We should not try to smooth his path too much, or to lift him to this goal; encourage him to climb to it. And, further, the way to accomplish this end, is go on just as we have done, building up to the literature and the science of ceramics. The rising generation will surely come to regard this literature and this science as the foundation stone of their industry. And in time it will accomplish just what we are impatiently desiring to see it do now. Rome was not built in a day. But in a generation, or two, we shall see ceramics lifted from the position of a crude trade up to that of a refined and highly technical art.

Speaking only for myself, I think one of our greatest dangers is a too rapid growth. We do not want to take in members faster than we can make them think and feel as we do on these questions of the policy of the organization, or its ultimate ends. We want members, whenever we can get them, if they are of the right sort. People who believe as we do, whether they are technically trained or not, are a help to us. But I am not in any hurry to get into the Society, a large number of men, who see dimly, if at all, the advantages of maintaining our work on the highest possible plane, and who will expect us to make our proceedings come to their level. Rather, let us take the position which we have so far held; that of maintaining first and highest of all, our own ideals; and second, of offering the right hand of good fellowship to any who, seeing those ideals, believe them good and desire to travel towards them in our company.

"UNWEATHERED vs. WEATHERED CLAY"

BY

EARL McK. OGLE.

In presenting this subject to the society for discussion, I do so to bring out the observations and experiences of the members, rather than to advance any particular belief of my own, although I am free to state that as far as making tile, by the process now in most general use, is concerned, I am in favor of the unweathered material.

The broad statement which we sometimes read, and frequently hear, that all clay products would be better if made from weathered clay, I think is erroneous and entirely misleading. This statement should be qualified by several exceptions, before it can be swallowed without pain, though I am ready to admit that in the manufacture of all fire clay materials, where it is necessary to gain plasticity and to remove as much of the alkalies and contained salts as possible, it does improve the texture of the clay to have all the materials thoroughly weathered. This is also true of many classes of pottery, where the materials are merely mixed in a semi-wet state and pugged, but where all the materials are thoroughly mixed in the slip tubs and are then lawned, dried, ground and repressed, I believe that the advantage lies with the unweathered clay, and that it produces a body more uniform in texture and color than the weathered clays.

It is of course recognized, that all clays do not exist in the same condition or formation, and therefore each clay or bed of clay may require a different manipulation, but I am of the opinion that nearly every clay that can be slipped or washed up to a creamy state, will produce better results when unweathered than when weathered. In using the word *weathered,* I mean *thoroughly weathered* clay; not a pile of clay fifty feet square, that has been exposed to the weather

for a year, and which is frequently called weathered clay, when as a matter of fact not over eighteen inches or two feet of the exposed surface could possibly be affected in a year's time.

My reasons for believing so strongly in unweathered clays are as follows:—when clays are taken fresh from the pit, they frequently contain pyrites, lime, alkalies—etc., which upon prolonged exposure are decomposed, and washed out, or by decomposition stain the entire mass, whereas if they are immediately washed, lawned and dried, only a slight per cent. of the alkalies or fluxing materials are removed, and the various minerals in the form of sulphates will be removed entirely when passing over the lawns. This would therefore tend to give a more uniform mass than could be obtained by the weathering process, and if we are to depend upon an analysis for compounding our bodies (the analysis being usually made from the bank or pit or unweathered clay) we would likely be lead astray in using the same formula, after the clays were weathered.

Quite recently to settle the matter for myself, I used a common buff clay, which many people had told me it was impossible to use without weathering. I took the pains to make two tile which are herewith exhibited, one from clay fresh from the bank, on the back of which you will find marked "New," the other from clay which had been thoroughly weathered for considerably over a year. In order that the conditions might be exactly the same in handling both clays, they were washed up separately at the same time, lawned through the same size screen or mesh at the same time, dried the same number of hours side by side, were ground to the same fineness, were pressed with exactly the same pressure at the same time, being made in a double die so that there could be no possibility of any unevenness in pressure. They were then fired side by side in the kiln, and there seems to be from a causal observation no apparent difference in either of the samples, but I think that the unweathered clay makes slightly the best tile. The white marks on the face of this tile, are due to the fact that on the platnum there adhered some of the clay of another color

which was being used at the time that the trials were pressed, so of course these marks should not be taken into consideration.

I have tried the same experiment with red, salmon and chocolate clays and in every case the tile made from unweathered clay was generally of a more compact texture and better finish than those made from weathered clay.

The uses of clay are so varied, as well as the formation of the beds of clay, and every pottery uses a different process in the manipulation, that it is not possible to lay down any general rule that is applicable to all, but I am confident if this subject were more fully investigated and these experiments or trials compared, that a considerable saving could be effected to our manufacturers as well as to improve the quality of the articles produced.

In conversation with many people on this subject, the opinion seems to be that the same or better results could be obtained if the clays were slipped and lawned immediately on being taken from the pit, but that the expense would add considerably to the cost of manufacture.

While I have no figures to present as a basis of calculation, I am of the opinion that the first cost of slipping is not perceptibly greater than the cost of weathering, and when the further fact is considered that a larger per cent. of first class goods could be made from the clays so treated, I believe that the slipping process would be cheaper in the end, for the greater part of the manufacture of clay products is not so much in the cost of the material, or the first manipnlation of same, but is due largely to the loss in the kilns, and in imperfect pieces, so that in fact it seems to me poor economy and a very short-sighted policy to try and save a few cents in the cost of manipulating the raw material, to lose dollars from the effects of same in the finished product.

As to the question of ageing or allowing the clays to rot after being blended into a body, this is an entirely different proposition, and I believe that it is now conceded that this does improve the character of the manufactured articles. The principal reason advanced by the French ceramists for this, is that the organic matters contained in all clays, when

subjected to ageing conditions in a damp place or in the presence of water, are decomposed or undergo a sort of putrefaction. The evolution of gas which takes place while fermentation is going on, causes a movement or readjustment of all the atoms or particles contained, and therefore produces more plasticity and homogeneousness, and this causes the body to shrink less and more regularly in the fire.

The organic matters which exist in the clay, are destroyed by spontaneous combustion under the influence of moist air; this at the same time reacts on some traces of sulphates which are found there; and changes them into sulphuretted hydrogen, while changing into carbonates at the expense of the surrounding carbonic acid. Salvetat's explanation of this is, that the sulphuretted hydrogen developed in this process is formed by the sulphate of lime changing into sulphuret of calcium under the influence of certain organic matters, and escapes when this sulphuret comes in contact with the carbonic acid in the air.

The above however does not disprove that weathered clays are better than unweathered clays, but on the contrary, I believe tends to confirm the position I take, that where clays are washed on removal from the pit, they produce a better and more uniform class of articles than the weathered, and my reason for so believing, is, that washing and lawning through a proper mesh will remove the larger per cent. of organic matters contained, as it is a well known fact that the refuse caught on the lawns, contain a very large per cent. of organic matter; this, if allowed to weather, would have decomposed to such an extent that a greater part of it would have passed through the lawns, and been incorporated into the articles made therefrom. When it is taken into consideration that even a hair will cause very serious flaws in fine pottery, we can see to what an effect organic matters, if contained in any quantity, would produce. It only remains to be determined whether the weathered clays contain less organic matter than the unweathered clay, which is thoroughly slipped. This I have not been able to demonstrate on the account of lack of time, as well as the delicate mechanism necessary, but I hope and trust that some member of

the Society will be interested enough in the subject to do so, and present the matter to the Society with a synopsis of the tests at some future meeting.

DISCUSSION.

Mr. Stanley G. Burt: Mr. Ogle's paper touches on a topic on which I had it in mind to say a few words a little later.

Mr. Ogle divides the topic, as I would have done, into two heads, weathering and ageing of clay. I had intended to consider the weathering, but I agree largely with what he says on that subject; but in the ageing of the clay, Seger attributes the consequent development of plasticity, to a different cause. He attributes it to the development of acidity in the clay. He took clay and mixed it and allowed it to settle: took the same clay and added a little alkali to it; and in a third sample, he added a little acid. And he shows that the one to which he has added the acid, has increased in plasticity, and the one to which the alkali was added has lost plasticity. And he argues that the traces of alkalinity which the clay originally carried is overcome by the acids generated by organic decomposition; and that is one of the main features obtained in ageing.

We, in this country, do not as a rule worry ourselves about the ageing of clay. When I was in Europe, I noticed that the ageing was considered of the greatest importance. The clay is stored in cellars and kept at least six months. In the Royal Berlin Porcelain Works, there was a magnificent long cellar, where the clay was piled up four feet high, and they never think of touching it under six months.

The Chair: Is that filter-pressed clay?

Mr. Burt: Yes.

The Chair: Has it been pugged or rolled?

Mr. Burt: No, it is piled up just as it come from the press. Mr. Ogle to my mind correctly divides the subject into two heads weathering and ageing. I agree with him that there are many cases in which weathering is not only unnecessary, but absolutely injurious. It is on the other half of the subject, however, that I wish to ask a question, that is in reference to ageing. Now the European potters age their

clay bodies, that is let their clay mixes stand in damp cellars for months. As far as I could find out, the great object of this was to increase their plasticity. This it undoubtedly does. Seger has explained to us that this result is due to the generation of acid in the decomposition of the organic matter present. What I am anxious to find out is what is there to be gained besides plasticity in ageing a body. The mixes I am speaking of have been carefully washed, lawned and pressed.

Mr. Edward C. Stover: Professor Hart, of Lafayette College, has examined weathered and unweathered clays from the standpoint of bacteria. He claims it is a case of fermentation, in the cellars where the clays are ageing; and regarding this statement which Mr. Burt gives us, about the acid and alkali, wouldn't the acid generated simply increase the tendency to fermentation?

Mr. Ernest Mayer: When a man tries to make out that unweathered clays are better than weathered clays, he has got a contract. There are some classes of clay ware, that we cannot make without weathering and ageing the clay. I do not think it is right to assume that the case is proven, by the means of these two samples. Unfortunately Mr. Ogle is not here, so we cannot ask for more details about these two samples. There ought to be more trials made, before coming to any conclusion.

The clays from which vitreous china is made, all stand in cellars, and I don't think those wares could be made without ageing. There are some factories in East Liverpool being built now, with magnificent cellars, for the clays must be held sometime; the longer, the better.

Mr. Alfred Yates: It depends largely from what standpoint of clayworking you argue. Referring to plastic flooring tiles, I would say, that clays ought to be finely ground and weathered before pugging.

Plastic flooring tiles made of weathered clays are made superior to those made of newly ground and pugged clays. In surfacing flooring tiles with slip, better results, are obtained by using aged slip, it gives a smoother surface, and better finish. By using new slip the coating will be irregu-

lar and there will be more or less cracking on the face, showing more shrinkage in one part than another and much rougher finish than produced with aged slip. Therefore, ageing slips and weathering clays in my opinion is a decided improvement for this kind of ware.

Mr. H. B. Skeele: It may be interesting to the members of the society to know that in the manufacture of roofing tiles at the extensive Broseley tileries in Shropshire, England, they do not use any clay that is not a year old. I saw them handling clay, a year's supply at a time, on a large platform on which the clay was lying about 5 ft. in depth. A two-inch water pipe ran the full length of this platform above the middle of it, and from this the clay was kept constantly wet. At the end of six months they turned it over by hand, throwing unweathered lumps to the top and after six months' more treatment they considered the clay ready for use. They seem to think they can't make good ware out of clay that is not handled in this way.

Mr. Yates: From experience I have had in making shingle tiles in Staffordshire, England, I will say that it is impossible to make as good tiles from new stock as from weathered clays.

Mr. Thomas Gray: I recently had a little experience along this line of ageing clay. We had some clay on hand for six months which was finally made up in ware. This ware was carried in the unburnt condition in a wagon over cobble stones for about two miles, and we only lost fourteen pieces. The man who made the ware said if they had been made with the clay taken from the press, as we always use it, it would not have stood the trip without considerable loss. But the clay was very tough—he had worked the clay himself, and he said it was aged. He spoke of it as being a very tough, nice clay to work with, and very much improved by ageing.

Mr. Cannan: I think the dry press process of making floor tile cannot be compared to the manufacture of plastic ware. When you slip a tile, you don't care for plasticity, and I think this discussion is not taking into account the differences in the process of manufacture.

Professor Orton: This is a very large topic which is being discussed. There are subjects for four or five papers in it. One point which I think ought not to be confused with others, is the effect of weathering on breaking down the fossil structure of the older clays. Anyone who will go into the face of a shale bed, and make some paving brick from the crop clay and then work under the hill where it is unaltered by weathering and make some more brick by the same process as before, will find out the difference. The weathering has done something to the clay, which cannot be duplicated without time and trouble. That is one point to develop which properly, would require a paper of considerable length.

Another point which has been touched on a little, is the development of objectionable impurities in the clay during ageing—the conversion of sulphides into sulphates, which are soluble. There is much to be said pro and con on that point. There is no doubt that it does take place. If you are going to use a slip process at the conclusion of the weathering, by which the bulk of the sulphates are taken into solution and removed, you get a final material practically free from sulphates. I think the weathering under such circumstances is questionably advantageous. If, on the other hand, you are going to work a material by a process which will give no opportunity to wash out the soluble substances, it is well to work it as quickly as you can after mining, before weathering sets in, and expel the sulphur from the sulphides by roasting it out in the kiln.

A third subject of inquiry is the effect of the washing process on the plasticity of a clay. Every stone ware potter will tell you that he can "throw" clay after a pugging process, but not immediately after slipping and filter pressing. If they attempt to throw the clay, or turn it from the pugged material, just as it comes from the filter press, it lacks coherence and toughness to a remarkable degree. The throwers always insist on the filter-press plates being partially dried out, and then throw into the tempering pan, and reground with a little more water, and so brought up to a tough condition. If this is not done, they say that each grain appears to

remain independent of its neighboring grains, instead of forming with them a fibrous or cohesive whole.

Then the point which Mr Stover brought up, is a subject about which more than one paper, could be written without exhausting this field for speculation. I refer to this theory of plasticity being influenced by bacterial growth. Some may scout this idea, but I do not. I believe there is a great deal in it. If I am not mistaken, that matter was discussed before us before. All of us have plenty of instances of a slip set away for a short time becoming sour and offensive. It contains nothing but the ingredients of the clay suspended in water. The water is not especially impure, yet there is something either in the clay, or the water, or in both, that furnishes material for fermentation and decay. These changes are accomplished with the usual signs of fermentation and decay, viz.: change of color and bad odors. Yet unquestionably there is an increase of strength and toughness in the clay resulting from this treatment. Take the glass-pot makers; they prepare their clay one or two years in advance. They keep beds of plastic pugged clay from four to eight feet thick, over the whole bottom floor of their factory buildings. It is so short when first pugged, that they could not use it; but after aging a year or two, it develops a fibrous structure and great toughness. But, when it is aged, it smells offensively. I believe bacterial growth has taken place, which has rearranged the grains of the ingredients, and permitted them to come together, as they did not do when forcibly compacted in the filter press; and the new structure is desirable, where the old structure is not.

REPORT OF THE COMMITTEE ON SEGER CONES.

Mr. President and Members of the Society:

One year ago, you appointed a committee of which I was designated as chairman, to take up the subject of certain alleged defects in the behavior of the lower members of the series of Seger cones, or to be more accurate, that portion of the series in which oxide of iron is a constituent (No. 3 to No. 010). These defects had been pointed out in a paper by our Mr. E. E. Gorton (in Volume II, pp. 60-73), and substantiated in part, by the experience of various members of the society, who discussed the paper. The committee was instructed to look into the subject, and report back its findings to the society.

The committee has, for various reasons, been unable to meet as a body, though some communication has been had by mail, and some exchange of samples for observation and analysis. I have personally been able to give the subject some little attention, and in the absence of a formal report of actual committee work, I submit the results of my observations as follows:

THE USE OF IRON AS A FLUX IN THE CONE SERIES.

Mr. Gorton's paper claimed the iron in the cones was the cause of numerous irregularities, and thought its use was a grave mistake.

I can fully agree with the opinion given that the use of iron in the cones is objectionable, and there can be no doubt that the fusibility of these cones would be affected, depending on whether a reducing or oxidizing fire is maintained. Outside of this, I do not see any objection, nor do I believe that the other defects cited are due to the iron.

In an experiment I made on this point I replaced the iron with oxide of zinc, leaving all the other materials the same, and making all allowances, but these cones failed in the same way as those described in the original paper.

ON THE CHANGE OF THE CONES COMPOSITION BY ACTION OF THE KILN GASES.

From the description given of the irregularities of the cones, such as drying up, and bending at the base instead of gradually bending, and afterwards from examination of the cones themselves, I was fully convinced that a part of the trouble at least was caused by the volatilization of the boric acid, as I had stated in the discussion of Mr. Gorton's paper. From the statements given by him, that such failures as described did not occur when burning in either a small muffle kiln or an open kiln, and also that no other persons present had had a similar experience except that which I gave in my discussion of the subject, and from the fact that some of the kilns which Mr. Gorton was using were the same as some of our own, led me to believe that the cause for the trouble with the cones lay in some peculiar condition in the large muffle or terra cotta kilns.

In kilns such as described in the original paper, the heating is done by conduction and radiation, and this is necessarily slow, and, as the muffles are seldom tight, a portion of the combustion gases find their way in. This passage of kiln gas through the muffle would not be objectionable were it not for one fact, and that occurs when burning fuel containing sulphur. In an examination of the vapor and gases from a kiln, it was found that these were distinctly acid and it was here that the trouble was located. In another experiment to determine what action the acid vapors had on the cones, it was proven that the fritt, which enters into the composition of the cones, was attacked and partially decomposed. In a laboratory experiment, it was found that sulphuric acid of a strength as low as .05 per cent. readily decomposed a part of the fritt.

It will readily be seen that as soon as a decomposition of this kind occurs, especially at a low temperature, and in the

presence of moisture, there is no reason why some of the boric acid should not be carried away.

Another material affected by the acid vapors is the calcium carbonate, the greater part of which is converted into calcium sulphate, and this, in combination with the loss of the boric acid, seems to my mind, to fully explain the failure of the cones.

From an examination of some cones Mr. Gates sent me, I found the tips to be burned to a hard mass, with little evidence of vitrification, corresponding to the dry coating of the exterior. The base of the cones had melted, but only in the interior, showing plainly that this part had not been affected, and giving evidence that the action was a slow one, working from the surface inward. Naturally the tips or smallest part would be most affected.

It will be seen on reference to Mr. Gorton's paper that he had overcome the difficulty in his own case, by dropping the iron from the cone batch and adding an increased proportion of a boric acid fritt. Cones made in this manner, fired in the same kiln as before, went down in a slow gradual curve from top to bottom, and did not grow hard on the tips or surface and did not swell at the base.

If my theory that the loss of B_2O_3 by exposure of the glaze to an acid-impregnated atmosphere is correct, it may be asked, why did not the cones with the additional boric acid show the same or more pronounced defects.

If we will remember the action that takes place in the kiln, the evaporation of the water from the ware, and also from the kiln itself, and the combination of this water with the acid gases from the combustion products, then we have these acid vapors constantly at work slowly decomposing the fritt, and as it is decomposed, some of the boric acid is carried off.

From many analyses made on material where this action has taken place, it was invariably proven that there was a shortage of boric acid.

But it was also found, that where the vapors attacked the cone mixtures, that a protective coating was built on the exterior of the cone, consisting of alumina, silica and calcium

sulphate, which protected the cone from further action of the acid vapors.

In another experiment, taking the cone mixtures of Mr. Gorton and subjecting them in thin layers, or as a glaze, to the action of the kiln gases, it was found that they failed to mature, that is, the lower layers fused to a perfect glass, while the top was dull, dried and blistered. On examining the last mentioned glazes it was found that they contained over two and one-half per cent. of SO_3 which of course would be combined with the calcium.

Again looking at another side of the question we have a frit of low melting point and by increasing the frit and dropping the iron, chemical combination will begin to take place at a lower temperature.

But in the cones containing the oxide of iron we have a mixture with less of the fritt, but with a mass of finely divided sesquioxide of iron, which separates the particles, and has a tendency to keep the cone open, and in a porous condition, and which is one of the last elements to enter into combination, thus giving every opportunity for the acid to attack the fritt and the lime.

To prove the matter in another way, we took the same samples before mentioned, and after firing them in the same kilns with fuel containing little sulphur (for instance oil) and with wares which have not a large percentage of non-oxidized sulphur, none of the troubles spoken of took place, but instead all glazes and cones matured in their proper way and were bright and clear.

My conclusions therefore are :

First. That in the Cremer cones or those in which a boro-silicate fritt is used, the oxide of iron introduces an element of uncertainty into the mixture which is unnecessary and that it can well be dropped.

Second. That the oxide of iron is not in itself the cause of the irregularities in the behavior of the Cremer cones, when used in muffle terra cotta kilns.

Third. That the real cause of the difficulties given is the expulsion of boric acid from the cones by sulphuric acid generated in fuel gases and also the conversion of calcium

carbonate into calcium sulphate in the same manner. By these two changes, the ratio of fluxing ingredients is very seriously interfered with, so that the cone no longer melts when it should, nor melts in a normal manner when it finally does go down.

I therefore recommend the use of some other frit, which will not be so readily attacked by acid vapors, and that either little or no free lime be used in the construction of the Cremer cone series.

Respectfully submitted,

A. F. HOTTINGER,

Chairman Committee.

DISCUSSION.

Mr. Stanley G. Burt: I would like to ask Mr. Hottinger whether he has analyzed a fired cone, to determine what per cent. of boracic acid was lost?

Mr. Hottinger: I can not give it by figures. I never made any direct boracic acid determinations, but I could tell about what the loss was.

Mr. Burt: You showed a clear loss from the cone?

Mr. Hottinger: Yes, a clear loss.

Mr. Edward C. Stover: That loss of boracic acid in the cone, may possibly account for some changes in boracic-acid fritt-glazes. We would have the same results under the same conditions as you did with the cones, and that would seem to lay the blame on the kiln gases for some of these defects.

Mr. Burt: I think undoubtedly a good deal of probability is added to Mr. Hottinger's theory of the volatilization of the boracic acid, from the fact that this effect is not noted where we have a rapid fire. If we have a rapid fire carried to the conclusion, the cone appears to behave very well. But in the long, prolonged fire, we get the true opportunity for the volatilization and probably get that effect.

Now, regarding the suggestion of adopting an entirely new type of cone. This present cone, undoubtedly, is bad. The aim of the committee should be to get something better. There is no question, but that the Cremer cone is a miser-

able mixture. Those of us who are using a lower fusing glaze, have right there the means of making a cone, which would serve them very well. We have a glaze maturing at say, 05: It is comparatively easy to advance to say 01. And I would be glad to know whether the committee has gone further with the work and is ready to offer a substitute cone ?

Mr. H. B. Skeele: It may be of interest in regard to this question to state simply that we are burning our kilns with oil, burning off in forty-eight hours, using cone No. 08. It melts out in the middle, and runs out at the bottom, sometimes standing reasonably upright in the middle.

Mr. Alfred Yates: I would like to ask some information, being brought up in the old school and accustomed to using trial pieces. I would like to hear what is the advantage of the use of cones over trial pieces ?

Mr. Ernest Mayer: I believe one of the greatest advantages to be derived from the use of the cone is this. I do not suppose there is a man here who has not some time had trouble with his kilns. The fireman knows all about its going wrong before you do, if he knows his business. One of the first questions you ask the man is, "Where are your trials?" He has them all ready for you—You don't know whether they are one day, or five years old, or whether they are all out of one trial hole. He will say, "I don't know what the trouble is—here are the trials I fired by." He can't do that with the cones. There they are, and they tell their own story, whatever it is. He can't fix them: he can't take them out, or put them in: he can't get at them. That is one of the greatest advantages.

The Chair: He can knock them over, if they won't go down.

Mr. Mayer: I had a fireman once who, if the cone did not go over when he thought it should, would shove it over with the poker. But it did not make a particle of difference, for they would always tell their own tale anyway. (Illustrates on blackboard.) A cone which goes over naturally will go over something like this (in a curved shape); but a cone which has been shoved over, will have a break in it

right there (indicating), which will show that it has been shoved over every time. It will bend nearly at right angles. I charged my man with it, and he admitted having shoved them over several times; but there is nothing about this that will fool anybody twice. If it is shoved over, it always breaks it; but if it comes down by the fire, it will come down gradually like that (indicating). There is no question about that.

Mr. William D. Gates: Suppose the cones begin to get too hot: can't a fireman regulate that to a slight extent by letting in cold air?

Mr. Mayer: I suppose it could be done to a certain extent.

Mr. Thomas Gray: I differ with Mr. Mayer as to the cracking of a cone which is pushed over. I have showed Professor Orton one of his cones, which was not pulled over, but which was cracked.

Mr. Mayer: I did not say it would not crack except by being shoved over. That very often happens. In making up the cones, if there is the least tiny crack in a cone, it will magnify in the burning and become worse, and many of them do that. But the appearance of a cone which has been pushed over is entirely different to any which have come down by the fire. I have used them for a little over five years, and have never had a mishap in any way with them.

Mr. Burt: There is another point in favor of the cones. As I understand the trial piece system, the ordinary method is to take a ring and spread it with the glaze; and during the progress of the burn, you draw out that ring, and endeavor by estimating how the fusing is taking place in the glaze on that ring, to judge the character of the fire of the kiln. I have used that test for years, tested it and used it, and it is after all a mere question of guess work. Once fused, there is nothing to tell whether you have gone too far, or whether you have an over-fired kiln. With the system of firing by cones, you say, "Cone so-and-so represents my heat"—say cone one. You put in cone one, and also put in cone two. If cone two comes down, you know the ware is spoiled. How can you know that with trials. The glaze may show

well up to four or five cones above the proper heat. We know many glazes, which mature at cone 05, which are beautiful to cone 5, but the ware which bears them will be injured. In firing with slip trials, it is purely a question of judgment. One man will say, "Here we have a fairly brown color on that, just about the shade of color we generally fire to." Some one else interested in the firing may come along and he thinks it is not quite what is wanted. It is only a question of guessing as to whether you have the proper shade. With the cone, you know it is down and what that means. There is ground for argument in the former case, but no room for argument as to when the cone is down.

Mr. Lemon Parker: I want to know whether fifteen or twenty per cent. more or less of silica in the different classes of clays will have any effect on the behavior of the cones? I mean the material in the kiln itself.

Professor Edward Orton, Jr.: If I understand your question, I would answer that the cone is picked out in reference to the ware you are going to burn, and it responds to the same law the ware does; in both, the process is one of silicate fusion. You select a cone which fuses at a temperature where your ware is in such a state of vitrification as you desire. It may be porous or vitrified. Different persons use cones for different ends. The cone measures the *heat-work*, and that is what you want to know. You do not care about *temperature* as such; you want to know what the temperature has accomplished in the way of changes in the clay body.

The cone is nothing more than a modified and improved trial piece. It is an empirical method, not exactly a scientific one, but it tells you better than any scientific instrument yet invented, what conditions you have in your kiln. It is influenced by the same agencies that the ware responds to, and tells more closely when a given result is attained than any mere temperature instrument,

For Mr. Yates' benefit, I will say that Dr. Zimmer read a paper at the first meeting on this subject, and the paper, together with the discussion which followed, were published in Volume I of our transactions. And in Volume II, you will find this question pretty thoroughly gone over once more

as to the advantages and the disadvantages of the cone system.

Someone said here this morning that the principal difficulty is likely to be found in the members of the series below cone four, where iron or boracic acid is used. I think the statement is probably true, that the principal difficulties are found there. Though I make and sell cones, and it might be thought to my advantage to keep their difficiencies out of sight, I will say that I believe that the truth is more profitable than anything else, and I welcome these discussions because they always bring out some point not hitherto noticed.

I have had repeated instances where complaints have been made of cones failing to melt at the proper point. In one case, which was submitted to me only about two weeks after the last meeting, I had in mind the suggestion which Mr. Hottinger made last year as to the influence of sulphuric acid, and looked for it. The cones sent to me were numbers nine and ten, and it was stated that they had come down, side by side. They were blubbered and blistered so that we could not tell which was which. The user complained that the cones showed no distinction as to melting point, and that his ware was injured on account of it. I sent them to the laboratory and had them analyzed, and found *two and one-half percent of sulphur.* I also had found on inquiry that a muffle kiln had been used. I had studied the operations of a muffle kiln on other occasions, and I had observed the extremely strong smell of sulphur in the atmosphere of the muffle, as compared with the smell of the gases proceeding from an ordinary kiln.

The atmosphere in the muffle is not subject to the same lively movement as in an ordinary kiln; consequently the ware fired in a muffle is to a certain extent *soaked* in the muffle atmosphere, and if this atmosphere contains more sulphuric acid than usual, it is natural that wares containing free calcium carbonate will take it in, and their melting point be affected. I think, therefore, that where muffle kilns are used, and cones are used, a distinct effort must be made to ventilate the muffle, even at the expense of the fuel used.

Dull or blistered glazes are likely to result from the same causes as do dried up and swelled cones. It seems to me a ventilated muffle is almost necessary if we expect to get good results. This is one difficulty in the use of cones, but it is a point which the user, and not the maker, must overcome.

In regard to cracking, Mr. Mayer, who makes and sells cones in the open market, just as I do, says that cracking is always likely to take place. One or two gentlemen here have alluded to the fact that they have had difficulty in that way. So far, I have had the most trouble in keeping my cones free from cracking, in those numbers in which the proportion of clay is lowest. Cone four requires very little clay. As we go up to twenty, we get more and more clay in each number, and have to use less and less binding material, to make the cone solid and strong. From four to nought two or nought three, the amount of clay used is very small, and we must depend wholly on artificial binding material to hold them together, and it is very difficult to prevent air-checking where this is done. If checking does occur, the cones are liable to crack open in the firing. But it is easy to tell the difference between a cone which goes down out of its place by cracking, and one which goes down in the normal way. If it goes down by cracking, it goes down quick and at an angle. Part of it will be straight, very frequently. Whereas, if it goes down in the normal way, the curve will be slow and gentle.

The fact will remain, when the last word pro and con has been said on Seger cones, that no other form of heat-measure has yet been invented, which will tell so exactly what you are accomplishing in your burning. It is an invaluable adjunct to every ceramic process, but it ought not to be used to the exclusion of other means of heat measurement. I have never advocated giving up the old trial pieces or rejecting the Le Chatelier pyrometer,—both have uses. If you can afford to buy a pyrometer, use it by all means; but the cones will still give you information, which you can not get from the pyrometer.

Mr. Parker: If we have magnesite brick on one side of

the kiln and fire brick on the other, would not that affect the behavior of the cone?

Professor Orton: The fact that you have got a basic brick on one side of a cone and an acid brick on the other side, would not affect the cone itself.

Mr. Parker: Aren't the cones of a silicious nature and does not the gas carry basic matter? The reason I asked the question is that I have noticed when we had silicious fire brick in the kiln with magnesite brick, one would be glazed from the vapors thrown off by the other. We would find the fire brick glazed, while the magnesite did not seem to be affected.

Professor Orton: If you will look up an article written by Dr. Cramer on the "relative effect of the fluxes," and published in the Thon Industrie Zeitung for 1896 or 1897, you will find that he shows that almost every ingredient entering into ceramic compositions is capable of appreciable volatilization, and he could even show by analysis the amount of each element lost by firing.

Mr. Parker: Your cones are silicious, are they not?

Professor Orton: Yes; but I do not think that the losses by volatilization would be so great as to affect the temperature at which the body of the cone melts.

Mr. Parker: The cones do get coated, but I don't know whether the coating affects the heat at which they fuse or not.

Mr. Bloomfield: Some years ago, I thought I would do a little experimenting with a small kiln, making magnesite brick. I made some magnesite brick, and burnt them in a kiln (not knowing any better) lined with ordinary fire brick made in New Jersey. I did not have any lining left to that kiln. The magnesite destroyed the clay brick, and I don't see why it would not affect the cones.

Mr. Burt: I think in that case the volatilized matter would settle on the face of the cone, and we would have possibly a slight trace of vitrification there, which would seal the inner part of the cone from the approach of the kiln gas. I am anxious, however, to see the society return to my original question which has been lost sight of, as to whether it is within the province of this committee to suggest and

offer some substitute for the lower cones, and whether it has some suggestion along that line.

Mr. Hottinger: I was somewhat in doubt as to what the committee was expected to do, or should do, in the matter of suggesting a new cone-series. I have made one or two suggestions, to use a more stable fritt in making the cones, and to eliminate the free or easily decomposable lime.

Mr. William D. Gates: I want to urge that this committee act further. Unfortunately I did not hear all the paper; but there certainly should be something done by the Association regarding the lower numbers of the cones. It seems to me Mr. Hottinger has been left too much alone in this work. Of the cones sent to us to burn under coal-burning conditions, only a few of them were burned; the rest were put upon the shelf when we returned to burning oil, and did not get burned.

There is no question about the value of cones. There is nothing but what a man can doctor in a kiln, but he can do less with cones than anything lese. We have been left in the position where we use trials, cones and pyrometer; and the burner, if the pyrometer did not act as he thought it should, and if the cones did not act as they should, has been very much inclined to go back to what he thought best, the old reliable trials. But that is because of the difficulty with these lower cones. We cannot tie absolutely to one thing. I do not know just exactly what is to be done in this matter, but I had great hopes that something would be accomplished by this committee. I reasoned that if the sulphur was the damaging agent, and would affect the cones, it would also affect the glaze, but I have never followed this out.

Mr. Burt: I would like to make a motion. I move that the report be received and the committee be continued, and the Chair empowered to appoint additional members if necessary, and the committee asked to continue the work and offer a substitute for the Cremer cones. (Seconded)

The Chair: Just one remark, before I put that motion. We don't want to get in the habit of organized committee work, to the exclusion of individual work. Professor Orton

made appropriate remarks on this subject twelve months ago, to the effect that when a line of this sort opened for investigation, it was in order for each member to appoint himself a committee of one, to follow that investigation; and I think that is the right policy to follow. A man investigating on his own account is very much more at liberty than one who has to consult a colleague. We all stand ready to render each other assistance when it is wanted, and if one of us has an idea, he can follow it out better alone than if he has to go hand in hand with somebody else. I put that to you as my view. I don't want to refuse to put the motion, but I think it wiser to let it stand that any man is at liberty to investigate along these lines if he is disposed to do so.

Mr. Burt: I want to say what has been said before on that subject by Mr. Stover,—'what is every man's business is no man's business'; whereas if a committee is entrusted with an investigation of this sort, it is still empowered to call on other members for their experience and help in the work, and thus we will arrive at something.

Mr. Mayer: The sentiment that what is everybody's business is nobody's business is strictly according to my idea of the situation. If the old committee is continued and the chair asks somebody else to act with it, I believe the work will be done and a report made to this society at the next meeting.

The Chair: I am very glad to have elicited these points of opinion. It has been moved and seconded that the committee be continued and the chair have power to add to its number, and that its report be presented at some future time, preferably at the next meeting.

(Motion adopted.)

Mr. Skeele: I want to call attention to the fact that Mr. Burt's motion called for the development of a new series of the lower number of cones by this committee. I hope this portion of Mr. Burt's motion will not be overlooked.

FURTHER NOTES ON THE DISTRIBUTION OF COST IN THE MANUFACTURE OF WHITE-WARE.

BY

ERNEST MAYER.

Last year I gave the Society my ideas as to a method of determining and distributing the cost in the manufacture of white ware, by the use of which a potter could intelligently figure the cost of any piece of ware he has to offer for sale, and also determine what articles are most profitable to sell, and what articles to avoid as far as possible. The first paper was complete as far as it went, and gave the method by which I have been able to trace out the cost in my own works.

This year, I desire to present a more careful analysis of the various sources of cost in manufacturing white table-ware. It is given solely for the purpose of showing the final distribution, the amounts in dollars and cents that the various sub-divisions of labor and material actually reach in a years business.

I believe such calculations are useful in many ways; they will certainly tell their own tale; I will indicate by a few examples the ways in which I think it of use. Suppose a man in locating a pottery, having first decided the probable out-put, wants to know at what advantage or disadvantage he will be, in a new location away from the recognized pottery centres. Knowing from these figures, the amount of coal, clay, &c., on which he will have to pay freight, he will be able to tell exactly where he will stand.

This calculation will also show the cost of production; which items are the principal ones to attack, with a view to making more economical production, either in material or

labor, and in the question of the application of the machinery to the industry, the figures given will show exactly the amount that can be saved; provided the capacity of the machine is known in advance. A careful study of these figures will reveal much other information that it is impossible to indicate here. I am certain of one thing, viz: that any pottery manufacturer who will take the trouble to work up a similarly exact table of cost distribution for his own plant, will find that he knows vastly more about his plant and his business when he gets through than he does now, and in my own case, this information has many times over repaid the labor spent in getting the facts together. No one can tell where the leaks in any industrial enterprise are, until he has followed them out by methods like this or which attain the same general end.

The following tables have been prepared as representing the cost of manufacture of the following quantity of white ware.

40000	dozens	.....handled tea-cups
40000	"	.....tea-saucers
40000	"	.....seven-inch plates
40000	"	.....six-inch plates
40000	"	.....five-inch plates
40000	"	.....four-inch fruit saucers
40000	"	.....individual butter-dishes
6666	"	.....covered dishes, eight-inch
3333	"	.....open dishes, eight-inch
3333	"	..... " dishes, twelve-inch
3333	"	.....bakers, eight-inch
3333	"	.....pickle dishes
3333	"	.....boats
3333	"	.....covered butter dishes
3333	"	.....sugar bowls
3333	"	.....jugs—42
3333	"	.....bowls—80
316666		

or 40000 complete sets of assorted table-ware

Purpose for Which Expense was Incurred.		Amount in Dollars	Percent of Total Cost
Making	Potter	18016.00	12.3950%
	Mould Runner	1680.00	1.1558
	Batter out	1796.00	1.2356
	Finisher	4852.00	3.3381
	Handler	1080.00	.7430
	Handle maker	428.00	.2944
	Use of machinery (Pugging)	372.00	.2599
Clay	Material	12204.00	8.3968
	Stain	804.00	.5531
	Press cloths and lawns	224.00	.1541
	Labor	2232.00	1.5356
Moulds	Plaster of Paris	700.00	.4816
	Labor	1504.00	1.0347
Biscuit Firing	Placing	3132.00	2.1548
	Drawing	1440.00	.9907
	Fireman	452.00	.3109
	Oddman	1144.00	.7870
	Wad Clay	228.00	.1568
	Fuel	2964.00	2.0392
	Sand	64.00	.0440
Biscuit Saggers	Labor	420.00	.2889
	Clay	348.00	.2394
	Fuel (drying)	32.00	.0220
	Wash	4.00	.0027
	Washing (labor)	32.00	.0220
Biscuit Loss	Biscuit Loss	1716.00	1.1806
Glaze	Material	3668.00	2.5235
	Labor	376.00	.2586
	Fuel	72.00	.0495
	Grinding	296.00	.2036
Glost Firing	Placing	5616.00	3.8638
	Drawing	2076.00	1.4283
	Fireman	868.00	.5971
	Oddwork	1156.00	.7954
	Fuel	3048.00	2.0970
	Wad Clay	280.00	.1926
	Glost warehouse, dressing, selecting and putting away	2124.00	1.4613
	Biscuit warehouse, brushing and stamping	2684.00	1.8466
	Dipping	1988.00	1.3677
	Dippers' help	852.00	.5861
	Looking out ware for packer, including foreman of glost warehouse	1940.00	1.3347
Amount forward		84,912.00	48.4220

Purpose for Which Expense was Incurred.		Amount in Dollars	Percent of Total Cost
Amount brought forward.....		84,912.00	46.4220
Glost Saggers ..	Labor	1924.00	1.3287
	Clay	1020.00	.7017
	Fuel (drying)	84.00	.0578
	Sagger wash	244.00	.1679
	Bitstone	100.00	.0688
	Labor washing	180.00	.1238
Stilts and Pins.	Stilts and pins	920.00	.6329
Dead Expenses	Fire insurance	1408.00	.9687
	Boiler insurance	28.00	.0192
	Taxes	340.00	.2339
	Water	100.00	.0688
	Steam heat	792.00	.5449
	Lighting, natural gas	184.00	.0578
	Lighting, electricity	8.00	.0055
	Tool account	1344.00	.9247
	Repair account	2648.00	1.8218
	Office and management	1300.00	7.7744
	Watchman and foreman	2796.00	1.9236
	Expense account, advertising, etc.	1476.00	1.0155
	Sundry supplies	276.00	.1900
	Stationary	768.00	.5284
	Employers liability insurance	132.00	.0908
	Modelling	1320.00	.9081
Printing...	Labor—Printer	9760.00	6.7148
	Labor—Girls	13440.00	9.2467
	Engraving	2200.00	1.5136
	Color	1068.00	.7347
	Oil	200.00	.1376
	Paper	680.00	.4678
Harden-ing On...	Labor, placing, drawing, preparing-kiln, coaling, and firing, kindling and wheeling out ashes	2060.00	1.4173
	Bits	132.00	.0908
	Fuel	1608.00	1.1068
Total		145352.00	100.0043%

This quantity of ware will, after deducting the biscuit loss, loss in printing, hardening-on kiln, dipping, and kiln shed, produce in the glost warehouse, on an average, as follows:

78%=29640 sets of 100 pieces each, run of kiln
20%= 7600 sets of 100 pieces each, thirds
2%= 760 sets of 100 pieces each, lump ware and loss
38000 sets

In the above calculations, the principal materials used in the manufacture of this ware were found as follows:

Flint	440 tons
Feldspar	175 tons
China clay	410 tons
Ball clay	205 tons
Cobalt	400 pounds
Coal for biscuit, glost and hardening-on	3810 tons
Plaster of Paris	450 barrels
Wad clay	125 tons
Sagger clay (biscuit and glost)	342 tons
Borax	14 tons
Lead	8.5 tons
Whiting	10 tons
Printing color	800 pounds
Printing oil	88 gallons
The total cost of which is about	$31,960.00

Classifying the expenses in the above table, I find the following grand divisions:

Cost of labor	70.7721%
Cost of materials	21.3081%
Management and miscellaneous	7.7231%
	100.0043

FURTHER NOTES ON THE TENSILE STRENGTH OF CLAY MIXTURES.

BY

EDWARD ORTON, JR., E. M.

In Volume II of the proceedings of this society, pp. 100-125, an article was published by myself "On the relation between the tensile strength of clay mixtures and the size of grain of their non-plastic constituents," in which it was shown that in the experiments made, the tensile strength of the clay mixture varied inversely with the diameter of the grain of the sand, between the limits of .04 and .002 inch. But, another fact was also brought out, viz., that where a much finer sand was used, that a remarkable loss in strength occurred, which could only be explained on the assumption that the drying of the brickettes containing the fine sand had been too rapid, and had permitted cracks to be formed, by which the strength of the clay mixture had been reduced.

Obviously, such a supposition requires demonstration before it can be accepted, and the following work was done to see whether we could show by further experiment, that our hypothesis was correct.

Not having enough sand of the No. 6 size, the diameter of whose grains averaged 0.00017 niche, the first task was to make a sufficient supply. Mr. Frank H. Morrison of Denver, Colorado, was set at this work, and he prepared about 15 pounds of sand by the same general system described in the previous article. There being no intermediate sizes required, he proceeded directly to his goal, first floating out a 3 minute sediment, from repeated quantities of the sand, and next sedimenting this 3 minute material and refloating it for periods of 20, 18, 16, 14, 12, 10, 10, 10 and 10 minutes respectively. Thus no particle was used, which had not

floated in relatively still water for 10 minutes and a large proportion of the sand had floated for much longer periods.

This slushy impalpable material was then sedimented, collected in jars, treated with acid to remove metallic iron dust, of which there was considerable, even in this immensely fine pulp, then dried, screened to break up the caking formed by drying, and was measured by the microscope. The average diameter of the grains were 0.00024 inch. This is .00007 inch coarser than last year's No. 6 sand. Some of this difference is probably in the microscopic measurement, but the second batch of No. 6 sand was probably coarser than the first, as it was prepared by a shorter and more direct process.

The next step was to design a series of tests whose results should clear up the matter of this anomalous behavior of the clay mixtures containing very fine sand. Accordingly a large batch of the clay mixture, No. 6, containing 50 per cent. Mayfield ball clay, and 50 per cent. sand No. 6, was prepared by blending in the slip state, evaporating to dryness with frequent stirring, pulverizing and screening the dried residuum, and tempering the screened powder to molding consistency with water and vigorous wedging.

This mass of clay was now made up into brickettes. The process of manufacture was in all respects the same as heretofore, and Mr. Morrison made every brickette with the most punctilious care.

The brickettes were divided as fast as made among five separate series, whose treatment in drying was varied as follows:

Series No. 1: Dried in the laboratory, at a temperature of 75° to 80° F., in the open, and without precautions of any sort. Time of drying, nominally six days, but they were really dry long before they were tested.

Series No. 2: Dried in the same room, but was covered with a heavy thick cloth which was quite damp when first applied and which became dry only after 36 or 48 hours. Even then, it shielded the damp brickettes beneath from air currents and delayed their drying somewhat. Time of drying, six days, at which point they were tested.

Series No. 3: Dried in a closed box, about 18″ x 16″ x 8″ deep. The brickettes were placed in the box when quite fresh, and the lid was then nailed tight down and the air volume inside left to exchange its moisture with the outside air through the minute cracks in the box and pores of the wood. Time of drying, two weeks.

Series No. 4: These brickettes were placed in a box of the same dimensions, which had been previously washed, till the wood was well dampened. This box was then closed tightly, covered with a wet cloth and placed in a damp cool place and left to dry out slowly. The wet cloth and damp box would certainly delay the beginning of the drying of the brickettes for a considerable period. The box was kept in underground cellar for about three weeks or possibly four, until the brickettes had become solid and practically ceased to shrink. The box was then placed in the coolest corner of the laboratory and after another period was finally opened and its contents tested after eight weeks drying.

Series 5: These brickettes were placed on shelves in an air tight porcelain jar, provided with a lid which tightened down upon a rubber gasket by screw pressure. In the bottom of the jar was a mass of granular chloride of calcium, one of the most powerful dessicating salts obtainable. The brickettes did not touch the salt or come close to it. The lid being made tight, the jar was set in an underground vault and kept cool for eight weeks, during which time, the moisture of the air in the jar was rapidly taken up by the salt, but the drying must have been very slow, since the heat for vaporization of the water still in the clay, could only have been slowly furnished in this cool dark place. Examination of the brickettes from week to week showed them to be slowly progressing; they could be dented with the finger after four weeks drying. They were finally dried and tested at eight weeks, after having become perfectly solid and long past shrinkage.

Each series was finished by heating from three to seven hours in a hot air oven, at a temperature of from 120° to 130° C. This was done to expel hygroscopic moisture and to bring each series to a uniform condition. Mr. Morrison,

who performed all the work so far described, found that the temperature at which the brickettes were finally broken was important: that a brickette was weaker at 100° C than at room temperature, or 30° C, and distinctly weaker if allowed to thoroughly cool and take up a little hygroscopic moisture again.

Series No. 4 and 5 showed a vesicular structure in a marked degree. The probable cause was organic fermentation or decay, which was favored by the long quiet exposure to dampness. Three or four weeks certainly elapsed before any of these brickettes had become reasonably solid, and in this time, these processes probably fed on the organic matter of clay and water and generated the minute gas cells which caused the porosity noticed in the resultant brickettes. Series 1, 2 and 3 did not show this structure at all, but were made from the same lump of clay at the same time.

The series were broken in exactly the same manner and on the same machine as was used in last year's work, with the following results:

Series No. 1. Dried without precautions.

Brickettes—1	188.41	5 ...192.51
" 2	118.78	6... 172.08
" 3	159.74	7 ...184.32
" 4	204.80	8....155.64

Average—182.49 pounds per square inch. Fluctuation, excluding No. 2, which contained a large flaw, was 26.9%.

Series No. 2. Dried under a cloth.

Brickettes—1	172.08	5....167.98
" 2	176.12	6....184.32
" 3	192.51	7....208.89
" 4	135.16	8....188.41

Average—178.17 pounds per square inch. Fluctuation, 40.8%.

Series No. 3. Dried in a tight dry box.

Brickettes—1	188.41	5....188.41
" 2	192.51	6....159.74
" 3	143.36	7....168.84
" 4	172.08	8....200.74

Average—176.13. Fluctuation, 32.6%.

Series No. 4. Dried in wet box, in eight weeks.

Brickettes—1....	114.68	5....221.18
" 2....	172.07	6....225.28
" 3....	184.32	7....221.18
" 4....	217.08	8 ...192.51

Average, (rejecting No. 1, which was broken while too hot to handle, while all the others were permitted to cool down,) 204.80. Fluctuation, 26.0%.

Series No. 5. Dried over chloride of calcium, in cool spot, in eight weeks.

Brickettes—1. ..	143.36	5....212.99
" 2....	163.84	6....237.56
" 3....	163.84	7....229.37
" 4....	204.80	8....225.28

Rejecting No. 1, which was also broken hot, the average is 205.53. Fluctuation, 35.8%.

Collecting these results, we have

Series No. 1—Quickest, severest drying	182.49	pounds,	T. S.
" No. 2—Somewhat slower.....	178.17	"	"
" No. 3—Still slower	176.13	"	"
" No. 4—Very slow indeed.....	204.80	"	"
" No. 5—Artificial conditions.....	205.53	"	"

The maximum strength developed was 237.56 pounds per square inch, by the last process.

Comparing these results with last year's figures on the brickettes of corresponding compostion we find:

First. The lowest average of any of the five sets of brickettes is higher than the average of the brickettes made of coarser grained materials, viz.: 174.24 to 176.13. This proves that last year's work was abnormal, in some respect which cannot now be determined.

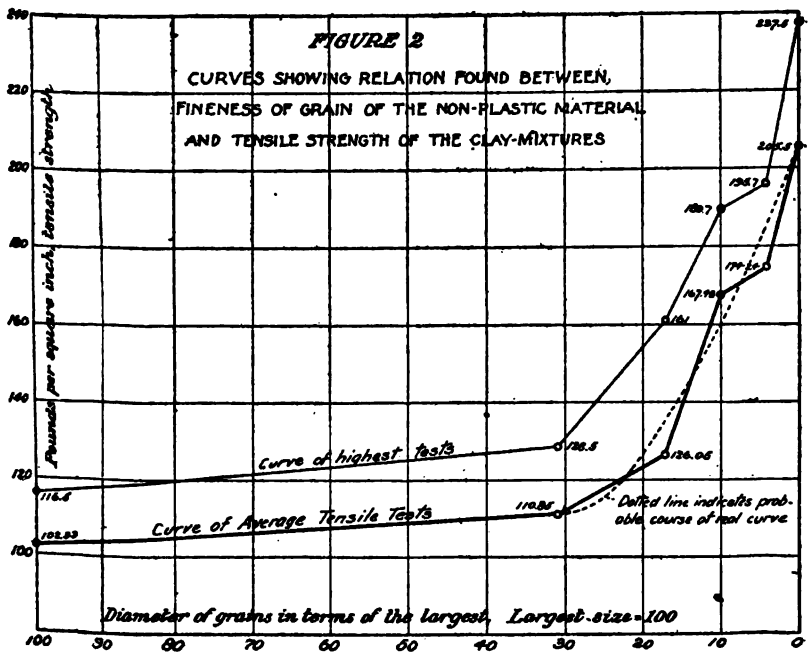
Second. The highest average of any of the five sets, which certainly cannot have developed any more perfect structure or greater tensile strength than the clay can be made to give in actual conditions, if the occasion justifies, is very materially above the last year tests on sand No. 5, viz., 174.24 to 205.53. This tends to prove the assumption given, viz., that faulty drying may materially alter the tensile strength of a clay brickette and that drying is therefore a feature which must be rigidly specified in any statement of

methods of determining tensile strength, and must be rigidly observed by all workers who expect their results to bear comparison with others.

Third. Repeating for the sake of convenience the results of last year's series, as amended by the fuller knowledge of No. 6 gained by this year's tests, we have the following:

Number of Mixture	Average Diameter of Grains of Non-Plastic	Average Tensile Strength
1	0.0364 inches	102.33
2	0.0114 "	110.85
3	0.0064 "	126.05
4	0.0038 "	167.43
5	0.0016 "	174.24
6	0.0002 "	205.53

The curve of last year, redrawn to show this new fact is given herewith.



Fourth. We may now safely amend our general conclusion of last year as follows:

The logical deduction from all of the data here presented and so far obtained, is that the tensile strength of mixtures of a plastic ball clay with equal quantities of non-plastic sands, will vary inversely with the diameter of the grains of the sand, from grains of 0.04 inch down to the finest sizes obtainable.

Fifth. Shorn of the obstacle of last year's work, and having now largely overcome the inconsistencies we then found, we may now believe, until it is shown to be untrue:

That the non-plastic ingredients of a clay influence its tensile strength inversely as the diameter of their grains, and fine grained clays will, other things being equal, possess the greatest tensile strength.

DISCUSSIONS ON ANNOUNCED TOPICS.

Defects of the Continuous Kiln of Today and Prospective Improvements.

Mr. J. Parker Fiske: After we have been soaring in the clouds with the chemist, in the beautiful discussions we have been enjoying on these technical matters, it occurs to me that when we come to consider such a homely, humble subject as continuous kilns, we may be brought down to earth with the proverbial "dull and sickening thud." However, I have no apology to make for proposing the subject now before you, because I believe, as I think all do, that the subject of kilns and burning is one of the most important practical subjects which can be brought up for discussion.

We know that the American manufacturer of ceramic ware is leading the world today in many departments. In the manipulation of his clay, in the forming of his clay into ware, and especially in the drying of that ware, he is certainly the peer, if not the leader of the world. I was much impressed with what Mr. Mayer said with reference to the English manufacturer who referred the Australian to America for the latest and best practice. It was certainly a wonderful tribute to what has been accomplished in America. At the same time, I think you will agree with me that in the matter of burning our ware, we are not on a plane with the practice of Europe, particularly that of the Germans. It is in the burning of our ware today that we are farthest behind. In the burning of our ware, we are not only subjected to a too great expense, but also to a great proportionate loss of our ware.

In looking over the possibility of improvement in this line, I find we have four types of kiln, viz., up-draft, down-draft, the so-called tunnel kiln, and the continuous kiln. Up-draft and down-draft kilns have been brought nearly to perfection. We can hope for little improvement in these lines. The tunnel kiln has never been brought to a satisfactory condition, so far as I know. Therefore, it is to the continuous kiln that we must look for any great improvement in burning.

I have been so much impressed with the importance of this subject, that I have devoted much time to the investigation and study of the subject in the last two years, not from the standpoint of an operator of the continuous kiln, but rather from the standpoint of one who has observed what other people are doing. I have been in position so that I could travel and visit almost every continuous kiln in operation in the eastern part of the United States, many of them two or three times. I have been very hospitably and kindly treated by those operating them, and they have given me the fullest data as to all important facts connected with their operation. In investigating those kilns, I have been impressed by several important points.

The first is, the great diversity in design among them. They have the Hoffman type, the so-called chamber kiln, differing somewhat from the Hoffman, and some others which cannot be classified. Some of these kilns are nearly a total failure, some are moderately successful and some are very successful. It must be said, however, that on studying results produced in these kilns, we are not nearly up to the plane of the German continuous kiln-user and burner; and the question has often been raised as to why it is that the continuous kiln has not been successful in America, while it is so well established in Europe, that not only common brick can be burned successfully, but front brick, terra cotta, roofing tile, and even high grade porcelain. In talking with one of our new associates, Mr. Voelcker, I was surprised and much impressed with the statement that they are burning Miessen porcelain in the continuous kiln.

It is rather hard to answer this question satisfactorily,

but there are few facts I would like to bring to your attention. In the first place, the American clayworker has not devoted himself to the study of the continuous kiln. He has been contented to invite his foreign co-workers in ceramic work to come here with their designs, and build kilns for him according to their own ideas; unfortunately, he generally has mingled a few of his own ideas with the foreign ideas, which may perhaps account for some of the failures. In America, we want to do things on a larger scale, more rapidly and more cheaply than anywhere else in the world. This perhaps accounts for much of our progress in manufacturing. But in regard to continuous kilns, I think it is one explanation of our frequent failures. Many of those kilns have been built too large, too quickly, and much too cheaply.

There are a few points which we can state as positive defects of the continuous kiln of America, and which have been largely overcome in Germany, as I understand from considerable correspondence with German kiln builders which I have had and from some reading on the subject. One defect of the continuous kiln in America is they have built the burning chamber too large—too high and too wide—so that we get a too great variation of heat between the top and bottom and front and back of the chamber. I was very much impressed with the articles Mr. Ashby wrote in "Brick" a year or two ago, giving some dimensions of English kilns, in which the chambers were very small, indeed; something we know little about here.

Another serious defect of the continuous kiln in America is the fact that the fuel almost invariably is distributed among the ware, which may be satisfactory in burning common brick, but will never be satisfactory for anything else. There is only one kiln in the eastern part of the United States in which the fuel does not come in contact with the ware. I refer to the Guthrie kiln at Union Furnace, Ohio. I am sorry Mr. Lovejoy is not here to speak to us on that subject.

Another serious defect of the continuous kiln in America is the fact that we try to work the kiln too hard. We try to get too much out of it. Instead of being willing to

carry away waste gases from the fire when absorbing a portion of the water smoke from the bricks in advance of the fire, we try to carry the waste gases so far through the green ware that it must condense in acid dew on the brick, thereby causing scum.

Another serious defect on the continuous kiln in America, which, so far as I know, has never been corrected here, but which has been avoided abroad, is the almost invariable use of underground flues. The underground flue is very bad in connection with any kiln, but in connection with the continuous kiln it is particularly objectionable, because we are dealing with comparatively cool gases; it is almost impossible to keep the underground flue dry and warm, so the draft will be good. It generally produces a poor and variable draft.

Another serious defect is that, in every kiln in this country, I believe, we take the water smoke from the bottom of our kiln, thereby carrying the condensing moisture to that portion of the kiln which contains the bricks which are sustaining the weight of the ware above them; and that is prolific of much of the trouble we have in continuous kilns.

It was with a great deal of pleasure that I have made the acquaintance yesterday of our new member, Mr. Voelcher, who has newly come from Germany with the very best German practice on the matter of continuous kilns, and from him I certainly expect great improvement in kiln building. I hope he will speak to us later on this subject.

I believe in speaking of the improvements which the continuous kiln must undergo, I may state a few as follows. We must build our kilns smaller, the chambers lower and narrower, so we can get more uniform heat from one part of the chamber to the other; we must build our kilns of a higher grade of construction, putting into them not necessarily better materials, but putting the materials together better, so they will be more permanent, and so they will be absolutely air tight, which is one of the essential features of continuous kilns and which has not received proper attention heretofore. I believe, also, we must be content to work the con-

tinuous kiln to a less degree, taking away the gases from the kiln somewhat warmer, before they have a chance to condense on the green ware. I believe we must take the gases away from the top of the chamber instead of the bottom, in order that the lower portions of the way may not be injured, particularly when subjected to a great weight of material on top of it.

There is another point which I think of great importance, but which has not yet been touched upon. I believe the continuous kiln must be erected by men properly trained for that work. I do not believe it will ever prove successful to allow the kiln to be built by people inexperienced in this class of building, even if they have the proper plans and specifications. The great success of the down draft kiln today, is due to the fact that the building has been in the hands of business men, who thoroughly understand their business, and undertake the building of these kilns as a business proposition under guaranty as to what the kiln will perform, with forfeiture clauses if it does not perform it. Unfortunately the continuons kiln has not been put forward by that class of men in this country, as a rule; consequently, there is not the confidence in that class of kiln which there is in up and down draft kilns.

I am a firm believer in it. I believe it is the coming kiln of America, as it is at present the most successful kiln of Europe; and I believe it is perfectly right for this society, for that reason, to devote a portion of time to the discussion of this question, to find out what are its defects today and what should be its improvements in the future. And if a society like this will give the subject such consideration, I believe the continuous kiln in America will be brought to the plane of the foreign kiln, and bring us as clay-workers to the front, not only in the manipulation and drying of clay, but in the burning of it; and thereby place us in the lead of the entire ceramic industry.

Mr. Alfred Yates: The object of using the continuous kiln in the past has been the economy of fuel. That object is undoubtedly accomplished by the horizontal draft continuous

kiln. The principal thing for which we should strive at the present time is to maintain this economy of fuel, and at the same time improve the quality of the ware burned in continuous kilns. I think the kiln of the future is the *down-draft* continuous kiln, in which we can burn fine goods and still maintain the same low cost in burning. The original continuous kiln was invented for the purpose of burning low grade or "backing up" bricks.

Mr. Voelcker: I understand that Mr. Yates has said the continuous kiln was used for burning "backing up" bricks. He is in error there. They can burn the finest pottery in this continuous kiln; they can make no better anywhere.

Mr. Yates: I had an experience of some eighteen months with a kiln, said to be an improvement on the Hoffman kiln. If you want to burn with a reducing flame, you have got to work on the down-draft principle. Whether economy can be maintained in the continuous kiln, maintaining the quality of the ware, is a question for the future. I don't know of any continuous kiln burning nice face brick, or pottery, as I left England twenty years ago. It was not done there at that time.

The Chair: That seems to be just the point; on this side of the ocean they do not have that success; and on that side, they do.

Mr. Fiske: We are after concrete, scientific facts, not generalities. All must admit that the continuous kiln is a success abroad, and it has not been a success in this country; although I wish to be understood as saying that there are some quite successful continuous kilns here, and when we consider those burning common building brick, there are quite a large number of successful kilns. But for high-grade ware, porcelain, etc., we know there is nothing burnt in this country in that class of kilns. I think it would be interesting for Mr. Voelcker to tell us the distinct differences between the continuous kilns in this country, built after the Hoffman type, and the kind used in Germany to burn Miessen ware.

Mr. Cannan: I would like to say that Mr. Ellis Lovejoy, at Union Furnace, is burning face brick, in continuous

kilns, and there is no doubt that he is making a success of it. He makes no distinction whether he burns in his down-drafts, or in his continuous kilns. For my part, I think the success of the continuous kiln lies in the proper manipulation of the draft, which is a matter which can be calculated according to the laws of physics.

Mr. Yates: What color goods does Mr. Lovejoy burn?

Mr. Cannan: Buff.

[The Chair at this point invited remarks from persons in the audience who were not members of the Society.]

Mr. W. A. Eudaly: I have found that there are very few things in connection with any kind of clay manufacture which are so attractive, and which siezes on us with such a terrific grip, as this idea of the continuous kiln. I never hear the subject discussed, that it does not take hold of me at once. Perhaps some of you don't know that I have spent the best part of my life in kiln manufacture, and kiln-study, and this thought invariably siezes on my attention.

I never have had the pleasure of going abroad, but I have no reason whatever to doubt but that abroad there are continuous kilns able to burn any kind of ware, because gentlemen say so, whom I know well. Yet, with all that, I don't believe we have ever had one in this country that would burn all kinds of ware, notwithstanding the fact, that many of the very best kiln builders the world has known have come to this country and built continuous kilns. (At least, we have been told that they were such.)

We have continuous kilns in this country that burn reasonably well. I mean by that, they burn common building brick very well. I don't know of a continuous kiln in this country burning very high grade pottery or white ware. I don't know whether in the old country Haviland ware and ware like that is burned in continuous kilns.

The Chair: The Miessen ware is burned in continuous kilns.

Mr. Stover: Mr. Griffin has told me that Haviland has just got in operation a successful continuous kiln for burning his ware.

Mr. Eudaly: Last year in a debate on this subject in

the brickmakers' association, Professor Orton said such ware was burned in the continuous kiln, with which statement I then took issue. I did not know it had been done. It has not been done in this country; here, they burn just common ware. Regarding this kiln at Union Furnace, it may be possible that they are now burning any of their brick promiscuously in that kiln, but if so, it is only recently.

Professor Orton: My impression is that they are using the continuous kiln for classes of work which they did not use to do.

Mr. Eudaly: Possibly; Mr. Lovejoy is a very progressive man, and I would not be surprised if he had made progress in that line. I know some time ago he did not attempt to burn any delicate shades in that continuous kiln.

Let me call attention to one thing: most all these discussions concerning continuous kilns, are made with this idea in possession of us. We are enamored with the idea of the continuous kiln, and we discuss it from that standpoint. We discuss it half the time, as if it was practical; we say what we say, with an "if", "but" and "and", and believe it is coming to pass; but we ought to stick closer to the actual proven facts.

Can we get the heat distributed? Not a doubt of it, but there is one serious difficulty to be overcome; sometime, someone will overcome it. In burning face brick in a continuous kiln, driving the fire round and round, as it begins to heat, it will lean to this fire and cause the brick to roll a little on the face. That is the great trouble with burning in a continuous kiln, to say nothing of chasing the fire round and round, to get the brick to stand and settle absolutely straight, and be uniform, straight and come out as they went in.

Mr. Voelcker: There is no building made of face brick in Germany that were not burnt in a continuous kiln. We have no trouble there in keeping brick straight and true. The relation between the flues and the draft, and the setting, are the main things in burning in a continuous kiln. You must set face brick right, and give them the right draft. See that it is set all right, and don't pile them up fourteen

or fifteen feet high and don't make the kiln too wide. The main part is to take your draft out above; if so, you have good results. As long as you take out the draft on the bottom you will get no good results.

Mr. Yates: I am sorry to trespass again, but there is one point I want to mention, in regard to people coming over here from the countries over there. I came here twenty years ago. Many things which they were doing in England successfully, failed here, when adopting the same process, same principle and method. I started to burning here without grate-bars, having been educated in Staffordshire where the potters never saw a bar. I found I hadn't the same coal and couldn't work it without bars. The conditions are different, and we must adapt ourselves to the conditions. I suggest that we study the down-draft kiln, which is not controlled by a current passing through horizontally, and I think we can utilize that principle in the continuous kiln.

Mr. Fiske: Speaking of Mr. Lovejoy's kiln, I don't think he would have any objections to my giving the figures in regard to its dimensions, which will go to illustrate the point which I stated here this morning. His kiln is a chamber kiln, divided by a permanent partition (illustrating on blackboard); these walls (indicating) make the chambers, which are in width about sixteen feet. The ware is put in here (indicating) to be burned; the fuel is put in through holes in the forward side of the chamber, and drops only in the fire-box. The air comes through here (indicating) and passing through the fire-box, passes up in this direction (indicating). It is something similar to the down-draft kiln, except his fire is on one side only. He uses the ordinary run of mine coal, I think.

Mr. Eudaly: Mr. Lovejoy had, and I think yet has, a supplementary furnace, which he fires from the outside.

Mr. Fiske: He has a wicket here (indicating) into which he shoves a few sticks of wood to dry out the ware a little, preliminary to putting the kiln gases in. Now, the point I am getting at is this. If we compare this with a downdraft kiln, we have got a kiln which is practically the

equivalent of a down draft kiln thirty-two feet wide. The exit for the gases is at that point in the center; attempting to burn with the fire on the side, the fire must pass in that direction to the opposite side, 16 feet distant, In a down draft kiln we have fires on both sides, meeting in the center. Of course, we appreciate the fact that a down draft kiln 32 feet wide would be a total failure, because the goods in front of the fire would be burnt to vitrification, and the goods in the center would not be burned. That is the principal trouble Mr. Lovejoy has had in his kiln. I think he has put in a permanent checker work in the bottom and rear portion, to take the place of the brick which he found were invariably soft at the end of a burn.

If I understand Mr. Voelcker correctly, if we could cut that chamber right in two, making it eight feet instead of sixteen feet wide, we would have an immense improvement over what we have today. That is the reason I have taken the time to draw these diagrams, to explain what was brought up this morning. To be successful, the continuous kiln must be built narrow; and that is practically to what success is due in the European continuous kiln today. As far as I have observed, in my travels, there is no continuous kiln in America built less than ten feet wide. Every one which is not more than ten feet wide is successful for burning common brick.

Professor Orton: Mr. Fiske's statement is too broad, I think, when he says that a down-draft kiln, 32 feet wide, would necessarily be a failure. I know of some down-draft kilns 34 feet wide, which have given satisfaction.

Mr. Fiske: I was referring to a square kiln; of course, in a round kiln they may be even 34 feet. I don't remember the height of Mr. Lovejoy's kiln; it is very high. The most successful continuous kiln burning today in America is ten feet wide by eight feet high.

The Chair: Are the German kilns for finer wares smaller?

Mr. Voelcker: Yes, sir. Our best German continuous kiln is seven feet wide and six feet high. That is the best kiln in the world. For my part, in the brick business I find

the Sieman & Rusk up-draft continuous kiln is the best. That is what everybody has got to look out for; the best for his particular purpose. The Sieman kiln takes all the smoke and steam out, drier and hotter. In other continuous kilns, the steam goes off at forty degrees centigrade.

The continuous kiln will burn good ware, if the man who handles that kiln, knows his work. It is generally not the kiln that is to blame. Sometimes it depends on the kiln. But if the continuous kiln was fed with pulverized coal with an automatic feeder, as Mr. Fiske's plans propose, I would expect a good deal from that kiln. The other matter suggested by Mr. Yates, the down-draft continuous system, I don't know anything about that. I know that when you take the draft off above, the steam can't condense to water on the bricks and you can't get whitewash. It is impossible. Down-draft, you might get whitewash.

Mr. Fiske: I think we would like to hear from Mr. Eisenhart, if we have time.

Mr. Eisenhart: I am not a member of this society. I am a brickmaker and don't feel that I ought to speak before this association. I don't know anything about fine brick, or fine pottery, but I have a continuous kiln in operation. I am satisfied from what experience I have had in burning common building brick, that one mistake in most continuous kilns is in having the chambers too large, and especially too high. As far as burning my ware is concerned, it don't make so much difference, as my kiln fires from the side almost exclusively. We don't burn any coal from the top holes until the bottom is red hot; then, when we throw a little coal, a good handful, in at the top, the minute it strikes the bottom it is gas; the finer the coal, the better the results. From what experience I have had with my kiln, I am satisfied that I could build a new kiln on that line, in which I could burn successfully face brick.

The Relation Between Kiln Atmosphere and the Color of Porcelain.

Mr. Griffin: I have asked to have this question put before the society, because I wanted to know more about it, not because I had anything to say. I would like some other member to tell me why it is that in burning porcelain it sometimes burns with a yellowish tinge and sometimes with a grayish tinge, the same ware. I don't pretend to know, myself.

Dr. William H. Zimmer: In regard to the influence of the kiln gases on porcelain, I would like to treat, first, of hard spar-porcelain. A kiln atmosphere can be reducing or oxidizing. The coloring matter in a porcelain body is the oxide of iron. The oxide of iron can be present as a ferric oxide, or as a ferrous compound, a silicate, which has rather a bluish white color. With ferric oxide, we get a yellowish tint. If we have a reducing atmosphere, we have all shades of white,—bluish white, grayish white, and a gray, which can actually be transformed even to a black by excessive reduction. This darkest color is due to carbon. When you get particles of carbon imbedded in your glaze, of course the black color that follows has nothing to do with iron, and is not to be referred to the iron.

If we come to the soft porcelains which bear a lead glaze, the influence of kiln atmosphere will be a little more complicated, inasmuch as the sulphur in the kiln atmosphere will have a more decided effect than on a leadless glaze. Of course, the coloring influence of oxide of iron will be just the same; you will have still the yellowish tint going over to white. But if your flame is too strongly reducing, it may reduce the lead in your glaze to little globules of metallic lead, producing a gray or blackish coloration.

Mr. Griffin: Then, do I understand that the whole thing resolves itself into the change of iron from the ferric to

the ferrous condition, this change assisted by the moisture which would be in the kiln?

Dr. Zimmer: Well, moisture might have some influence; it might assist it. Moisture especially has a great deal to do with influencing color if you have sulphur in the kiln gases.

The Chair: Doesn't moisture intensify any action there, whether reducing or oxidizing?

Dr. Zimmer: Yes.

The Chair: Few men have fired white ware, who have not come across blued ware accidentally when they don't want it. The first thing to learn in the remedy of a disease is to learn the cause of it. If you can produce your disease, you can cure it. Has the question been answered to your satisfaction, Mr. Griffin?

Mr. Griffin: Yes, sir.

Should Our Society Adopt Standard Processes For the Testing of Clays?

Mr. Charles A. Bloomfield: I would like to speak from the point of a man who has clay to sell. I have been selling clay for twenty-four years. While I have found in a majority of cases the man who buys clay is thoroughly honest, I occasionally find a man whose superintendent is a thief. A man deriving his income from another, who will not give a sample a fair test, unless he is paid for it, and who will not give an order for supplies unless he is bribed, is a thief. And more than once I have come across a man who had his own ideas about testing, which same were very far from mine or of any one else.

There ought to be, to protect the buyer and the seller, some standard method of testing clays, as to their fire-resisting qualities. Of course, there can be no absolute method for testing clays for every purpose. But, as to the

fire-resisting qualities, there should be a standard made and adopted by this society, which would protect every man here who is using clay, as well as he who is trying to sell it.

The Chair: I think it well to say that our committee on testing clays was organized to proceed on those very lines. There has been so much preliminary work to do, that very little has been accomplished in formulation of the actual tests themselves. But still, we don't propose to let the matter drop. I am chairman of that committee, but unless more rapid progress be made, I recommend that the society presently discharge me from that position, and put some one else in.

The work of the committee so far has been directed towards discussing what tests are necessary. We began with the analysis, but that goes but a short way. There are a great many considerations to be dealt with, and so far the committee has not completed the experimental work to show what tests shall be necessary, to say nothing of producing the tests themselves. That work of standardizing the tests themselves is a preliminary work, which has got to be done.

Mr. Bloomfield: Mr. Randall publishes a little book which he sells for one dollar, which contains the analyses of all the principal clays on the market. Regarding what you say about analyses, they are so similar to each other, that except for chemical purposes, one might say they are the same. Yet we know the clays themselves differ greatly, regardless of this similarity of composition.

Mr. Edward C. Stover: That point was brought out in Dr. Zimmer's paper. We get a chemical analysis of one clay which is much like another, but its action in practical work is very different, and the explanations in Dr. Zimmer's paper will account for some of these differences. I asked to have this question put on our list for discussion and when a man asks for information on a subject, Professor Orton at once puts him down for leading the discussion. I think a standard should be set for clay, so it will be understood by at least the men of the American Ceramic Society; not only a chemical analysis, but a rational analysis, and any other information that might be necessary to give as complete a

description of the capabilities of the clay as possible; and I had hoped the testing committee might have a report to make in that line at this meeting.

Note on a Crystalized Kaolinite.

Professor Edward Orton, Jr.: The plasticity of clay is generally believed to be a function of quite a number of different properties. One of these properties is the strong basal cleavage which kaolinite crystals exhibit. Another is the immensely fine state of division in which they exist; another is the greasy feel which they possess, smooth and soft. These are some of the properties which are considered as influencing plasticity. The theory of the basal cleavage of the kaolinite crystals in flat parallel planes, explains plasticity on the view that these plane surfaces will slide readily on each other, in lines parallel to their planes, but will not pull apart readily in lines at right angles to these planes. Grains roll on each other without forming large contact surfaces, while plates slide on each other freely yet are in contact over such large surfaces that they have the cohesive attraction which will not allow them to be pulled apart. Such is the theory.

A number of years ago, Dr. Bischoff published a description of crystalized kaolinite, and gave in his article, illustrations of some of the shapes which the crystals assumed. He said that this sample was from the National Bell Mine of Denver, Colorado; I have been accustomed to quote that article each year to my students, showing them the crystal outlines, and explain that kaolinite theoretically assumes these beautiful and regular forms.

Mr. Samuel Geijsbeek, one of our esteemed charter members, has recently gone west, and established a pottery in Golden, Colorado. In the course of his investigations, when he first went out to that country, he met most of the men who were reputed to know something about the clays of that locality. He met one man who stated to him accidentally that he had years before sent some kaolin over to Germany.

Mr. Geijsbeek at once became interested and his friend, on thinking further, said he thought he might find the rest of the original sample somewhere about his office. The kaolin, he explained, had come from out of a small cavity in the rock in his mine. Mr. Geijsbeek persevered with his questions until his friend finally searched the package out of an old desk, where it had lain forgotten for many years. The paper in which it lay was covered with dust, but inside it was all clean. It was the other half of the sample which Dr. Bischoff had worked upon. Therefore it has a historical and sentimental value. In this bottle (indicating) is part of it. You will see that it has a crystaline glint, and does not look like kaolin. Ordinarily, kaolin is a perfectly amorphous solid, and gives us no idea of crystalization. Ninety-nine hundredths of this sample is composed of perfect crystals.

I have brought a microscope with me and on its stage I have put a little of this crystaline kaolin dust. I want each member of the society to pass in review before the microscope and see some real crystals of kaolinite. The sight is as rare as it is beautiful. The basal cleavage theory receives a considerable support from this sample, from the fact that where a mass of kaolinite of any thickness is on the glass slide, it is easy by wetting it and squeezing it with another glass plate, at the same time sliding the glasses one on the other gently, to cause the thick mass of kaolinite plates to slide out on each other like a new deck of cards. One plate overlaps another, in succession, until what was a little pile of compactly placed plates has been spread out in a long train of overlapping scales. This takes place only when water or some fluid is there, to act as a lubricating agent. I have such an overlapping train of scales in the field of the microscope now, which I hope you will all note.

My assistant, Mr. Peppel, has just completed an analysis of this sample, which he did not know was a part of Bischoff's sample, in which he finds the following proportions:

Alumina	46.40
Silica.....	39.74
Water...	14.06

[Thereupon a short recess was taken to allow the members to examine the sample under the microscope.]

What Relative Economy Exists in Burning White-Ware With Bituminous Coal vs. Anthracite Coal.

Mr. Thomas Gray: If some inventive genius could produce a kiln in which either hard or soft coal could be burned without making a change for each kind, it would be a great benefit to potters who during the last year have seen the price of these two fuels fluctuate.

Although I have just repaired my kilns and fitted them for anthracite coal, I am rather in favor of the bituminous coal, for a difference of $10.00 or $15.00 per kiln looks large. I have not studied into this subject very closely and am unable to give any data, except that my kiln-man tells me that the loss of saggars in my bituminous kiln is very much larger than in the anthracite kilns.

In 1893 I fixed my biscuit kiln for bituminous burning and my glost for anthracite. The biscuit kiln for the last four years, has required constant patching, while the glost kilns have needed very little attention. Soon after repairing the biscuit, I gave up using it, and it stood nearly a year without firing, using one of the glost kilns for biscuit. When I repaired all the kilns in 1900, the two anthracite kilns seemed to be in the best condition, although I put about the same repairs on all. My foreman claims that taking the loss of saggars and the repairs into consideration, that burning bituminous coal is expensive, even though the fuel costs less per kiln.

I have been told by a Trenton kiln-builder that the Trenton potters were convinced that the anthracite coal was the fuel to use. I cannot say how that is, but perhaps some of the Trenton people who are here can.

I have used a Nova Scotia coal—Dominion coal—but found it to be very expensive, although more than $1.00 per ton cheaper than Cumberland. It is a good gas coal, but heats too rapidly, and "kills" the saggars and we had to

repair after each firing. The Boston and Philadelphia Face Brick Co. used it, and claimed they could save three to five tons per kiln. I have fired with oil, and this Nova Scotia coal has the same effect on the kiln as oil; the saggars crumble away very soon, seeming to lose their vitality, particularly those that came in direct contact with the oil or Nova Scotia coal.

Situated where I am, a kiln burning either hard or soft coal would be a great convenience, not only to myself but others as well.

Mr. William Burgess: My experience has been just the reverse of Mr. Gray's. We are farther away from bituminous coal than the East Liverpool manufacturers, who use it entirely, but we can get it for a dollar less than anthracite. In firing biscuit kilns with anthracite, and glost kilns with bituminous coal, I find less loss of saggers, and less loss in the wear and tear of the kiln. That is my experience.

Mr. Everett Townsend: We burn about the same amount of bituminous coal as of anthracite coal to a kiln, and, after keeping track of loss of saggers for several years. I can say there isn't much difference in the loss; it runs about the same with us.

Mr. Burgess: I think the wear and tear of the kiln is more dependent on the firing than on the fuel. When we commenced to use bituminous coal, we didn't know how to handle it very well; but as we become accustomed to it, we use it much more cheaply, and find it fully as satisfactory.

The Chair: Is your kiln up-draft?

Mr. Gray: Yes.

Mr. Edward C. Stover: There are a great many kilns of the down-draft type in Trenton, being fired with soft coal. I don't know which way the tide of opinion is running among the potters now, but they have changed some kilns to down-draft, and are using soft coal.

Mr. James Moses: I use altogether hard coal, and have had little experience with any other. I always thought the risk was very great in changing from one to the other, unless the fireman was used to the soft coal. I would like

to use soft coal, but the value of the ware inside the kiln is so great that I have always preferred to let other people experiment first.

The Chair: Mr. Moses evidently likes to use the cat to take his chestnuts out of the fire. I think the question could best be settled by finding how many kilns have been changed from soft to hard coal, and how many from hard to soft. My experience goes to show that kilns are frequently changed from hard to soft, but I don't remember of any changed from soft to hard.

Mr. Burgess: My experience is identical with that. Over at the Ceramic Art Company, they have just applied the soft coal process in their new kiln.

The Chair: I may lay claim to having some hand in that: I started that before I left there. I think you will find the reason that you find both fuels used side by side is that those in business who are successful think they will let well enough alone and not risk a change. The potter is a conservative person and rightly so.

Mr. Gray: At Baltimore, they use soft coal altogether.

The Chair: They never use hard coal in England; they use soft coal altogether.

IN MEMORIAM.

WILLIAM GOLDING.

It is with deep regret that we are so early in our history, called upon to chronicle the removal, by death, of one of our esteemed members. On April 11th of last year it was the will of Him, who doeth all things wisely and well, to call from the activities of this life

MR. WM. GOLDING, M. E., of Trenton, N. J.

Mr. Golding was born in Forest of Dean, England, on June 13th, 1846. He came to this country with his father while still quite a boy. At an early age he was placed to learn the trade of a machinist. At the age of 21 he entered the Polytechnic College of Philadelphia, Pa., from which institution, in the year 1871, he received the degree of M. E.

After graduation he entered the employ of the Pennsylvania Steel Company, of Steelton, Pa. His ability soon procured for him advancement to the position of Manager of the "Bessemer" department. In 1881 he resigned from this position to take charge of the Trenton branch of the Golding & Co.'s Flint & Spar Works.

When, in 1885, the corporation of Golding & Sons Co. was formed, Mr. Wm. Golding was made its Treasurer. On the death of his father, in 1898, causing a vacancy in the office of President Mr. Golding was unanimously chosen to fill this office, which position he was occupying at the time of his death.

Mr. Golding was held in high esteem by the community in which he lived. He was looked up to as a business man of uprightness and integrity ; a worthy citizen and a prominent and active member of the Methodist Episcopal Church.

In the death of Mr. Golding we feel that we have lost one who would have been an able supporter of this Association and one whom we all could soon have learned to call by that endearing name of "Friend."

WM. BURGESS,
ERNEST MAYER,
Committee.

[The above was adopted by a silent rising vote.]

Remarks of President Binns on the Occasion of the Elevation of the First Associates to Full Membership.

Gentlemen:—We have this morning a pleasing duty to perform, and, in executing it, I would like to call your attention to the fact that this society consisted at its organization only of charter members, who bound themselves together in mutual co-operation for the advancement of the cause of ceramic science. At that time, expansion was fully intended; but it was laid down in what you might call, perhaps, a tacit agreement, or unwritten law, that those who should be admitted to the ranks of membership in the future should win their spurs.

We have always welcomed associate members, but it was the general feeling, of which I think you all recognize the truth, that mere numbers do not mean strength; and, while we have taken the position of welcoming to associate membership practically all those who ask to join, in order that they may receive and in time give, yet it was and is the feeling that the policy and management of the society should be vested in full members only and that membership itself should be restricted to those who gain that position by merit, and by merit alone.

Two conditions are imposed. The first one is that of technical ability; we require that a man who aspires to place his name on the roll of membership, shall prove by some specific contribution on the floor of the society this fact of his ability to sustain himself there. The second requirement is that technical ability shall be supplemented by a desire to impart of his stores of knowledge and to serve his neighbor and mankind. In other words, that altruistic spirit for which the society stands shall be kept to the front. It is not enough to know; any one may do that. It is necessary to appreciate as well the beauty of giving. Unless a man is willing to contribute a portion of his knowledge to

his fellow members, here is scarcely the place where he will find his most congenial associations. It is well understood that many of us are the guardians of business secrets not our own, which we may not impart. But the proportion of our knowledge which the exigencies of trade and competition still require us to maintain in secret is small, and it is perfectly compatible with the interests of ourselves and our employers to impart knowledge on many lines of the greatest mutual benefit. No man can give without receiving; and we believe that no man should receive without giving according to his power and capacity to give.

The society has received applications from three associates for change of grade. This is the first time this has occurred, and I am sure that the members all welcome it with all the earnestness of which we are capable. The three associates whose applications for membership have been received are Walter M. Fickes, William Cannan, Jr., and Adolph F. Hottinger. Each of these men have presented contributions on the floor of the society. You have heard yesterday from Mr. Hottinger, whose contribution represents an enormous amount of work upon really useful lines. Only those who have been through such investigations know what work was involved in the few minutes talk Mr. Hottinger gave us yesterday in his brief abstract.

Mr. Fickes has presented to us, in lieu of the paper which he was prevented from delivering by the unfortunate accident which happened to him, his thesis which he presented for graduation in the Ohio State University. A resume of that was given to you by Professor Orton, and we in the council who examined it very carefully, came to the conclusion that it was work of a very high order, and the man capable of doing such work and imparting it, is the man we want.

Mr. Cannan you heard yourselves yesterday, and a similar remark will apply to him. His work is likewise of a high order, and we appreciate and value his candor and frankness in showing the results of his arduous labors.

I would like to say one thing in reference to two of these papers, for the purpose of encouraging those who may be

willing to present ideas. Last year I presented a paper upon mono-silicate glazes, which was crude in itself and unsatisfactory to me but in it was the germ of an idea. To that paper can be attributed the work of Mr. Cannan and Mr. Fickes. Here is encouragement for those who are willing to do only a little work; they may plant a germ which may grow to maturity in the mind of another man. We never know in this line "How great a matter a little fire kindleth."

The Council has decided to recommend for full membership these gentlemen, and herewith presents their names to this society.

Mr. Bloomfield: Mr. President; I move that Mr. William Cannan, Jr., be admitted to full membership in the American Ceramic Society.

(Motion seconded and unanimously adopted.)

Mr. Stover: I move that Mr. Walter Morgan Fickes be admitted to full membership.

[Seconded by Professor Wheeler; unanimously adopted.]

Mr. Gates: I take the greatest pleasure in moving that Mr. Adolph F. Hottinger be placed upon the list of full members.

[Seconded by Mr. Griffin; unanimously adopted.]

President: Will the Council please come forward? (Messrs. Zimmer, Orton, Mayer, Burt and Wheeler assemble in semi-circle in the rear of the President.)

[By direction of the President, Messrs. Stover and Bloomfield conducted Mr. Cannan in front of the Chair; Messrs. Lovejoy and Townsend conducted Mr. Campbell (selected to act as proxy for Mr. Fickes in these exercises) to same position; and Mr. Hottinger was escorted by Messrs. Gates and Moses.]

President Binns: Gentlemen, the Council and the American Ceramic Society, recognizing the merit of your work, and the enthusiasm which you have displayed in the cause of science, through its duly elected and accredited officers hereby declare you to be fully elected members.

We extend to each and all of you the right hand of fellowship, hoping that the ability you have severally shown in

your chosen field of labor, and the devotion you have exhibited to the common cause may prove to be but a beginning.

To some it has fallen to turn the hard and crusted soil and to cast in a handful of seed; but it is upon you, the men of youth and fire, that the burden of the fight must eventually fall. You will meet with discouragement, disappointment and perhaps failure; but soonor or later you will hear the rustle of the growing grain, and it may be that you will even catch the golden gleam which heralds an abundant harvest. It will come. You are working for the future, and such works demands an unusual measure of self-sacrifice. Be not cast down if the ripening seems to tarry, but learn with patience to labor and to wait.

For myself, I account it a peculiar honor that this event has occurred on the occasion of my occupancy of the President's chair; and I feel a warm personal gratification that while director of the second ceramic school in the United States, I should have the pleasure of welcoming as members a graduate and a student of the first school. Gentlemen, we welcome you.

Thereupon, the new members received the congratulations of the president and members of the council, and the continued applause of the Society. After a brief interval the business of the Society was resumed.

TRANSACTIONS

OF THE

AMERICAN CERAMIC SOCIETY

CONTAINING THE PAPERS AND DISCUSSIONS

OF THE

FOURTH ANNUAL MEETING

HELD AT

CLEVELAND, OHIO,

FEBY 10-12, 1902.

The Society is not, as an organization, responsible for the statements of fact or opinion expressed in its papers and discussions.

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CONTENTS.

Page

Officers of the Society for the year 1902 5

Membership of the Society 6

Rules 14

Publications of the Society 17

Preface to Volume IV 1

PAPERS.

1. Retiring address of the President 19
By CHARLES F. BINNS, Alfred, N. Y.

2. Notes on the Fine Grinding of Pottery Materials 25
By ERNEST MAYER, Beaver Falls, Pa.

3. Crystallized Glazes 37
By WILLIAM H. ZIMMER, PH. D., Coburg, Germany.

4. Stoneware Slips or Engobes 48
By SAMUEL GELJSBEEK, Denver Colorado.

5. Stoneware Glazes 61
By Ross C. PURDY, Columbus, Ohio.

6. Note on Fritting 79
By STANLEY G. BURT, Cincinnati, Ohio.

7. Opportunities for Research in a Factory 82
By CHARLES F. BINNS, Alfred, N. Y.

8. The Composition of Porcelain for Electrical Purposes 86
By ARTHUR S. WATTS, Findlay, Ohio.

9. The Use of Lead Ores in Ceramics 131
By HERBERT A. WHEELER, E. M., St. Louis, Mo.

10. Tin Oxide in Ceramic Fluxes 139
By STANLEY G. BURT, Cincinnati, Ohio.

11. Note on the Relative Point of Dehydration of Pure and Calcareous Clay..... 146
By WILLIAM M. KENNEDY, Beaver Falls, Pa.

12. The Manufacture of Artificial Sandstone.... 156
By SAMUEL V. PEPPEL, B. Sc., Columbus, Ohio.

13. Bacterial Growth as a Factor in the Ageing of Clay Mixtures..... 183
By EDWARD C. STOVER, Trenton, New Jersey.

14. The Equipment of a Modern Paving Brick Plant..... 189
By WILLARD D. RICHARDSON, Cleveland, Ohio.

15. Lead Fritts and their Adaptation to Pottery Glazes..... 200
By WILTON P. RIX, Newcastle, England.

16. The Structure of Silicate Mixtures.. 208
By HARRISON E. ASHLEY, S. B., New Bedford, Mass.

17. The Relation Between the Constitution of a Clay and Its Ability to Take a Good Salt Glaze..... 211
By LAWRENCE E. BARBINGER, E. M., Schenectady, N. Y.

18. The Constitution of Chromium-Tin Pink 230
By WALTER A. HULL, E. M., Bolivar, Pa.

19. The Fluxing Power of Mica in Ceramic Bodies..... 255
By RAY THOMAS STULL, E. M., Terra Cotta, Ill.

20. Some Gaps in Our Knowledge of Cements. (A note)..... 270
By ALBERT V. BLEININGER, B. Sc., Columbus, Ohio.

21 Notes on the Development of Greens from Cupric Oxide in Glazes..... 278
By FRANCIS W. WALKER, Beaver Falls, Pa.

OFFICERS

FOR THE YEAR ENDING FEBRUARY, 1908.

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ERNEST MAYER, - - - Beaver Falls, Pa.

VICE PRESIDENT,

FRANCIS W. WALKER, - - Beaver Falls, Pa.

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STANLEY G. BURT, - - - Cincinnati, O.

MANAGERS,

H. A. WHEELER, E. M. - - - St. Louis, Mo.
Term Expires, 1908.

ALBERT V. BLEININGER, B. Sc. - - Columbus, O.
Term Expires, 1904.

ADOLPH F. HOTTINGER, - - Chicago, Ills.
Term Expires, 1905.

HONORARY MEMBERS.

MRS. MARIA LONGWORTH STORER.
(Mrs. Bellamy Storer)
Cincinnati, Ohio.

MEMBERS.

BINNS, CHARLES F., Alfred, N. Y.	Director New York State School of Clayworking and Ceramics.
BLEININGER, ALBERT V., B. Sc., Columbus, Ohio.	Instructor in Ceramics, Ohio State University.
BURT, STANLEY G., Cincinnati, Ohio.	Rookwood Pottery Company.
CANNAN, WILLIAM, JR., Syracuse, N. Y.	Onondaga Pottery Company.
FICKES, WALTER M., E. M., Carnegie, Pa.	Rosslyn Brick Company.
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PURDY, ROSS C., Columbus, Ohio.	Private Assistant to Professor Edward Orton, Jr.
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WHEELER, HERBERT A., E. M., St. Louis, Mo.	Consulting Engineer Mining Metallurgical and Ceramic Work.
ZIMMER, WILLIAM H., PH. D., Coburg, Germany.	Redaction Sprechsaal.

ASSOCIATE MEMBERS.

ALRICH, JOHN C., Pittsburg, Pa.	Star Encaustic Tile Company.
BLOOMFIELD, CHARLES, A., 58 Quincy St., Brooklyn, N. Y.	Dealer in Clays.
BOWMAN, ROBERT K., Trenton, N. J.	Treas. Trenton Fire Clay & Porcelain Company.
BOWMAN, WILLAM J. J., C. E., Trenton, N. J.	Trenton FireClay and Porcelain Company.
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HAUGH, HARRY, Anderson, Ind.	Supt., Columbia Encaustic Tile Company.
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LINDER, MISS NORA, No. 528 Randolph Bldg., Memphis, Tenn.	Ceramic Decorator.
LINDEROTH, SVEN., Chicago, Ill.	Alhambra Ceramic Works.
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MADDOCK, JOHN, Trenton, N. J,	John Maddock & Sons.
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RIX, WILTON PARKER, 34 Liverpool Road, Newcastle, Staffordshire, England.	Consulting Ceramist.
ROESSLER, FRANZ. 100 William St., New York, N. Y.	Vice Pres,, and Supt., Roessler & Hasslacher.
RUSSELL, JOSEPH, 1302 Queen St., East, Toronto, Canada.	Brick Manufacturer.
SHAW, FRANCES D., 927 Edgewood Ave., Trenton, N. J.	Shaw Vitreous Enamel Co.

SIMPKINS, RALPH, St. Louis, Mo.	Manager Union Press Brick Wks.
SKEELE, H. B., Chicago, Ill.	Ludowici Roofing Tile Company.
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YATES, ALFRED, Johnsonburg, Pa.	Johnsonburg Vitrified Brick Co.

RULES.

OBJECTS.

The objects of the American Ceramic Society are to promote the arts and sciences connected with Ceramics by means of meetings for social intercourse, for the reading and discussion of professional papers, and for the publication of professional literature.

MEMBERSHIP.

The Society shall consist of Honorary Members, Members and Associates.

Honorary Members must be persons of acknowledged professional eminence, and shall not exceed five in number.

Members shall be persons competent to fill responsible positions in Ceramics and have suitable qualifications.

Associates shall include persons interested in Ceramics and the allied arts.

Honorary Members shall be proposed by at least five Members, approved by the Council, and receive at least 90 per cent. of the votes cast by letter ballot at the annual meeting.

Members and Associates shall be proposed by at least three Members or Associates, approved by the Council, and receive at least 75 per cent. of the votes cast by letter ballot. A candidate for admission must make application on a form prepared by the Council which shall contain a written statement of his age, professional experience, and that he will, if elected, conform to the laws, rules, and requirements of the Society.

All Honorary Members, Members and Associates shall be equally entitled to the privileges of membership, except that only Members shall be entitled to hold office and to vote. Applicants for a change in grade of membership shall conform to the requirements of a new applicant.

Any person can be stricken from the membership of the Society on the request of five or more Members, on the recommendation of a majority of the Council, if he fails to resign on the advice of the Council. Such person, however, shall first be notified of the charges against him, and be given a reasonable time to appear before the Council, or present a written defense, before final action is taken by the Council.

DUES.

Honorary Members shall be exempt from all dues.

The initiation fee of Members shall be ten dollars, and of Associates five dollars, which if not paid within six months after election, will render the election void.

The annual dues for Members will be fixed by the Council, but shall not exceed five dollars per year.

The annual dues for Associates will be fixed by the Council, but shall not exceed four dollars per year.

Any Member or Associate in arrears for over one year may be suspended from membership by the Council until such arrears are paid.

OFFICERS.

The affairs of the Society shall be managed by a Council, consisting of a President, Vice President, Secretary, Treasurer, and three Managers, who shall be elected from the members at the annual meeting, and hold office until the adjournment of the meeting at which their successors are elected.

The President, Vice President, Secretary and Treasurer shall be elected for one year, and the Managers for three years; and no President, Vice President, or Manager shall be eligible for immediate re-election to the same office.

The duties of all officers shall be such as usually appertain to their offices, or may be delegated to them by the Council or the Society; and the Council may at its discretion require bonds to be furnished by the Treasurer.

Vacancies in any office shall be filled by appointment by the Council, but the new incumbent shall not thereby be rendered ineligible to re-election at the next annual meeting to the same office. On the failure of any officer to execute his duties within a reasonable time, the Council, after duly warning such officer, may declare the office vacant, and appoint a new incumbent.

A majority of the Council shall constitute a quorum; but the Council shall be permitted to carry on such business as it may desire by letter.

ELECTIONS.

At the annual meeting, a nominating committee of five Members, not officers of the Society, shall be appointed, and this committee shall send the names of nominees to the Secretary at least 60 days before the annual meeting, who shall immediately forward the same to the Members. Any other five members may also present the names of any candidates to the Secretary, provided it is done at least 30 days befor the annual meeting. The names of all candidates, provided their assent has been obtained, shall be placed on the ballot without distinction as to nomination by the regular or an independent nominating committee, which shall be mailed to every member, not in arrears, at least 20 days before the annual meeting.

The ballot shall be inclosed in an inner blank envelope, and the outer envelope shall be endorsed by the voter, and mailed to the Secretary for collection. The blank envelopes shall be opened by three Scrutineers appointed by the Chair at the annual meeting, who will report the result of the election. A plurality of the votes cast shall elect.

MEETINGS.

The annual meetings shall take place at such time and place as the Council may decide, at which time reports shall be made by the Council, Treasurer, and Scrutineers of election, and the accounts of the Treasurer audited by a committee of three appointed by the Chair.

Other meetings may be held at such times and places during the year as the Council may decide, but at least 20 days' notice shall be given of such meetings.

Seven Members shall constitute a quorum at any regular meeting, and a majority shall rule, except where otherwise specified.

The order of business at the annual meeting shall be:—

1. Reading of Minutes of last meeting.
2. Reports of the Council and Treasurer of the Society.
3. Announcement of Election of Members.
4. Announcement of Election of Officers.
5. Appointment of Nominating Committee.
6. Old Business.
7. New Business.
8. Reading of Papers.

PUBLICATIONS

The Council shall act as a Publication Committee, and decide as to what to publish. The publications of the Society shall be sent to all Members and Associates not in arrears. The Secretary will furnish each author with reprints of his papers at cost price, provided due notice is given that reprints are desired.

The Society is not, as a body, responsible for the statements or opinions expressed in its publications.

PARLIAMENTARY STANDARD.

Roberts' "Rules of Order" shall be the parliamentary standard on all points not covered by these rules.

AMENDMENTS.

These rules may be amended at any regular meeting by a two-thirds vote of a letter ballot at the subsequent annual meeting provided a written notice of such proposed change is sent to each Member at least 30 days before said annual meeting. Said proposed amendments shall be printed on the ballot for officers and counted by the same Scrutineers.

PUBLICATIONS.

The publications of the Society are as follows:

DESCRIPTION OF VOLUME.	Price to Members.	Price to Others
Vol. I. Transactions for 1899, 110 pages, bound in paper,	$ 0.50	$ 4.00
Vol. II. Transactions for 1900, 278 pages, bound in paper,	2.00	4.00
Vol. III Transactions for 1901, 280 pages, bound in paper,	1.00	4.00
Vol. IV. Transactions for 1902, 300 pages, bound in paper,	1.50	4.00
Manual of Ceramic Calculation (contained in Vol. II as a part thereof,) 86 pages, bound in paper,	1.00	1.00
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Every member or associate receives one copy of the Transactions free for each year for which he pays dues. He is entitled to purchase copies for the years antedating his connection with the Society at the costs indicated in the first column. All others can obtain copies at the prices listed in the second column by sending order, accompanied with check, to

STANLEY G. BURT,
Care of Rookwood Pottery Co.,
CINCINNATI, OHIO.

PREFACE.

The following pages contain the papers and discussions of the Fourth Annual Meeting, held at Cleveland, Ohio, February 10th, 11th and 12th, 1902.

In addition, a new departure has been made in this volume, in publishing a number of articles and contributions which have not been read before the Society, and, in fact, were not prepared at the date of the meeting. This policy, it is hoped and believed, will be the first step towards relieving the pressure of our meetings, in which it has grown more difficult each year to hear all of the papers and discussions offered. As the Society grows in importance and influence, it will surely reach a point where only the papers of unusual general interest will be read at the meetings, and the others will be read by title and published. The progressive intricacy of the papers already presented has shown very clearly that it is only in occasional cases that masses of technical facts can be made intelligible by reading aloud, while they readily can be comprehended when put in print.

From this point of view, therefore, as well as the number and quality of papers, it seems that this is by all means the best year's work yet accomplished by the Society. In addition, it may be said that the volume would have been improved by the addition of two other articles, one of more than general interest to brickmakers, had not the time limits urgently required that this book be sent out without delay. It is expected that one or both of these articles will still be printed and distributed as a bulletin, between this date and that of the next annual meeting.

The growth of the Society has continued rapid and good. The members seem enthusiastic, and certainly gives one evidence of this fact in maintaining their connection with very few withdrawals for any cause. The continued increase in foreign membership is noted with gratification.

The announcement of the facts concerning the publications has been given a separate page in this issue.

EDWARD ORTON, JR.,
Secretary.

COLUMBUS, O., Oct. 1, 1902.

THE PROGRESS OF THE CERAMIC ART.

ADDRESS BY THE RETIRING PRESIDENT,

CHARLES F. BINNS, ALFRED, N. Y.

It is well, in any walk of life, as we pass the posts which mark our progress, to look backward and around, that we may take a broad and comprehensive view of the field we occupy. The work done by our competitors and neighbors should concern us at least to some extent and even if we clash in rivalry, we can find evidence of mutual interest and possibly help.

Progress can be measured from three widely differing standpoints: the artistic, the technical and the commercial. In most cases the final arbitrament is found in the last, for it can be said with some measure of truth, that art and science are successful in manufacture only in so far as they prove financially sound. But this not entirely true. Beautiful work is regardless of cost and no amount of money expended can of itself make an artistic product. The fact is also self-evident that artistic wares can be made at a financial sacrifice. In other words, a product may be artistic whether the manufacture pays or not, and a profitable line is not necessarily artistic.

The same is true to a less extent of technical success. To a less extent, because science is more readily controlled than art. The artistic faculty is subtle and elusive, the scientific is exact and material. But neither is scientific and technical success to be measured entirely by money. Our best research is carried on independently of financial considerations and an improvement in quality or manufacture is not necessarily a means of making money.

Commercial success, is of course, measured by the dollar. The man who can make the most profitable ware and sell the largest quantity of it is the most successful commercial man.

From these points of view then let us take a glance over the progress of the world in recent times.

It must here be noticed that in some of the most marked advances it is difficult to say whether the artistic or the technical is the more concerned. The former is the more obvious, certainly, but the latter is fully as important. The work displayed by the National manufactory of Sevres at the Paris Exposition of 1900 was the finest exhibit of hard porcelain ever shown. Technically perfect, it was artistically fine, but, of course, would be called a commercial failure. The Royal Copenhagen factory has work which is equally fine, though very different. This factory claims a new body for plates and dishes which is much lighter than the old, without any counter-balancing disadvantage. In such a matter as this, the technical, of course, overshadows the artistic, and in fact, for reasons already stated, the artistic advance is far less easily defined. Art is a matter not of a new discovery, but of a particular expression. It is, in fact, the soul of a production, while the technical is merely the frame or body.

In illustration of this, let notice be taken of the modern English wares. There is practically no technical variation in the product of half a dozen of the leading factories and yet some are more approved than others simply because of a finer artistic sense.

The minute variation, the subtle touch which may make or mar a work of art cannot be defined, nor is it to be located by any scientific method.

Of European technical advance we have little opportunity of judging as regards coarse wares, for there is practically no competition with us in the matter. In large measure, the older methods are used and the low price of labor renders the employment of modern machinery unnecessary. Hand-moulded brick are still in the lead in England and many of the lines of work would be considered antiquated even in the slowest American brickyards.

In fine terra-cotta, France takes the lead; the productions of Muller, of Paris, stand very high both in art and technique. Germany can match the work probably, but not the spirit. It is an interesting study for those concerned as

to why most German work is artistically so poor and most French so fine. Moreover, the Germans seem to be able to make more money out of their technical knowledge than the French of their art. Take for example the flood of cheap bric-a-brac with which the Germans are covering this country. One can but admire the technical excellence of the work, the composition of body and glaze and the simplicity of manufacture, while at the same time the paucity of artistic idea is most evident. These goods are sold at such trifling cost that when the sixty per cent. duty, the freight and the packing are considered, one wonders what price the manufacturer secured, to say nothing of the amount received by the wage-earner.

The production of ceramic wares in the United States is naturally of the more immediate interest to ourselves for not only are we concerned in the manufacture, but the success of our work will of necessity diminish the importation of competing wares.

Some of our manufacturers are, happily, free from the worry of foreign competition, but they will doubtless claim special and peculiar troubles. No day-worker has a monopoly of worry, but each claims to have a special heritage of it in some individual and peculiar right. The brick-maker has the field to himself—until his neighbor's territory is reached, but he has to give his product away in order to meet his pay-roll. The maker of finer wares gets a fair price, but the stores are filled with foreign importation, and so it goes.

It is probably without question that the manufacture of building material in this country is ahead of anything elsewhere. Brick of every description, common and front, glazed and enameled, fire proofing and terra-cotta, roofing tile and wall tile as well as paving brick are made here in great perfection and the lines of drain-tile, sewer-pipe and electric conduits run them very close. In fact, where the successful product depends upon an intelligent application of engineering, the American is well to the front. Here, the artistic element scarcely appears, except in the matter of terra-cotta, but the technical and mechanical skill involved is of a very high order. Mention should be made here of

the successful manufacture of porcelain baths and sinks. But a short while ago we imported all our supply of these commodities; but now, such is the advance made, we are exporting. It should perhaps be explained that "porcelain" is only a name. The body is a refractory clay which is coated with a white slip and a clear glaze, the whole being burned at one and the same time. The result is a compact, impervious ware which, if somewhat heavy, is practically everlasting.

In terra-cotta and wall-tile great advances have been made in the matter of glazes. Every prominent factory now has a full line of tints both mat and bright, many of them very beautiful. The problems involved in each case differ somewhat in that terra-cotta is glazed almost entirely upon the unburned clay, while tile being mainly dust pressed must of necessity be fired before glazing.

In the finer branches of ceramic art we are bound, if honest, to confess a certain weakness. There are many among the ranks of the connoisseurs who ask skeptically whether America can make fine pottery, and they emphasize their doubt by saying that the necessary clay is not found in the United States. We who are engaged in utilizing native clays know better, but given the material, can we make fine pottery? In reply, we point with pride to Rookwood but are asked if a solitary example demonstrates the ability of the nation. It does, provided the necessary conditions are observed. Here allow me to digress for a moment, not in defence of Rookwood—that were quite unnecessary—but in criticism and even condemnation of the shameless copyist, who imitates body, glaze and coloring and claims something just as good at one-third the price. I hold no brief for Rookwood and my criticism would be just as severe upon any copyist. He plunders the brain and thought of the master mind, he seizes upon the external features, those that can be easily and cheaply pirated. He ignores or slaughters the soul and all that makes the work really valuable, and carries out his impudent imitation only to make money. Do we not sometimes despair for the commercial morality of our people when such things are done and approved as being smart or clever. But there is a point of

satisfaction in this cruel competition: it is, that Rookwood by virtue of her superiority of resource, can move faster than the pirates, and in this is her real safety. Fine work such as this is spontaneous and exuberant, the originator soars triumphantly upward while the slow herd of copyists flounder laboriously below.

In the manufacture of household pottery there is matter both for regret and congratulation. The wares classed by the hired girl, conveniently and comprehensively, as "dishes," are produced here in large quantities, and the matter for regret is, that they are generally so devoid of artistic merit. A glance through the advertising pages of any trade journal will reveal shapes, classed as "new" which are simply a rehash of similar things and which are evidently modeled in view of the sagger and stilt. Just what will make easily, fire safely, and hold something inside while in the kiln.

Nevertheless the manufacturers may be congratulated in that the use of American materials is extending, the bugbear of crazed goods is disappearing and the works are being run at a profit. There remains, naturally, some considerable number in the ranks of the non-progressive and even of the wilfully blind. Every now and then the ceramic world is startled by the announcement that a remedy for crazing has been found, a magic solution which, added to the dipping tub will banish forever the phantom of craze while, had the discoverer only read the current technical literature, he would long ago have found that magic is quite unnecessary and that science has already exorcised the demon and only an imaginary ghost remains to those who keep their eyes shut.

The greatest advance of recent years is in the matter of hotel china. This is a distinct outgrowth of American enterprise and skill. There had long been a dissatisfaction with French china. The ware was brittle and took long to replace. When, therefore, the American potter came forward with a tough ware — not an imitation, but a native success— he was welcomed by the hotels using large quantities of table goods. By one factory after another the problem has

been taken up and solved. Harder fires gave stronger wares, better clays gave better texture and color, improved methods gave better glazes until the American hotel china in technical quality and fitness for its purpose stands unrivalled in the world.

Once more it is laid upon me to raise the lament that we do not make fine porcelain. Hotel china, Belleek, Faience, each has its place but the lover of American ceramics cannot handle fine specimens of European wares without a pang of regret. Why is the like not made here? We have the finest clays, the best workmen, the greatest wealth. Why not then the finest porcelain? Will no patriotic manufacturer take up such a work *con amore*? Will no enthusiastic millionaire endow a small factory to remove this blot from our industrial and artistic fame? Surely, when millions are spent in collecting specimens manufactured long ago in other lands there should be a possibility of the artistic and technical skill of our own people being fostered and encouraged ro this end. Be it remembered that fine wares cannot be made for money alone, that profit must not be thought of—at least in the beginning—if a world-wide reputation is to be made, it will be apparent why our manufactures have not touched the question. For the same reason it must be evident that if we are to make porcelain which will be equal to Rookwood faience, which will compare with Copenhagen and with Sevres, it must be beneath the roof of some beneficient capitalist who will draw his profits from the pride of possession.

The members of this society may congratulate themselves that their share in the advance has not been immaterial but still more upon the fact that when a call to greater advance comes they are ready.

Much of the work has been done in the face of cavil and misrepresentation. The incredulous smile of the "practical man" still lingers around us, and there is much opposition to be lived down but the day will come, and it is not far distant, when the graduates of our schools will be sought by every plant in the country and our industry will rise to heights that are as yet unexplored.

NOTES ON THE GRINDING OF MATERIALS USED IN EARTHENWARE BODIES.

BY
ERNEST MAYER, BEAVER FALLS, PA.

The question of the degree of fineness to which the flint and feldspar used in pottery bodies shall be ground is one that does not receive in the United States the attention and care bestowed on it by European potters. It is my intention, as far as I am able, to awaken a greater interest in this subject, and show by trials submitted for your inspection, that the statements made can be amply verified.

As to the system of grinding in Europe, more particularly in England, where I gained my practical experience, let me point out a fact not generally understood, viz: That dry grinding is practically unknown—(remember, I speak of England). Although the system of dry grinding has been systematically tested and a considerable sum of money spent to make a success of it, it has been abandoned as impracticable, and they still continue to grind their materials by the wet process as of old.

Before proceeding further in my subject, it may be well for me to state my reason for bringing this subject up at all and explain the reason why I lay such stress on the matter of fine grinding. I recollect of no meeting held by our Society where this subject has not been brought up, and at several of them I have have had considerable to say, and when the subject was talked over in one of our discussions last year, I made the assertion that it is one of the most important enconomic problems in our business, as I hope to show before my paper is concluded.

It has been to me, till recently, an unsolved enigma, why English potters can fire a dense body, with a clear ring and a close fracture, at a temperature of Seger cone 5, whereas, our body at a temperature of cone 8 down and 9 begin-

ning to move, will not make so dense a ware, or so clean a fracture, while we have a body containing, as shown by chemical analysis, a percentage of alkali and fluxing materials, at least 35 per cent. greater than theirs. I don't wish to be misunderstood and have it supposed for an instant that I am comparing American and English ware to the detriment of the former, because the fact is now appreciated and has been for some time that we are getting just as good a price as they do in open competition.

What would it mean to us today, if we could bring our biscuit heat down to cone 5 instead of going to the excessive temperature we do?

First: A very important item, saving of fuel.

Second: Saving in saggars, on account of less heat employed, and the fact of being able to use very much cheaper clays than the very high priced ones we now have to buy in order to get sufficient refractoryness.

Third: The easier regulating and more even firing of the kilns at the lower temperatures.

This third reason is unquestionably a debatable one and one that I don't consider so important as the first two, as, from my point of view, there can be no possible question raised concerning these. I also claim that the finer grinding of our materials will not only help in reference to fuel and saggers and firing, but it will stop a large quantity of so-called mishaps that occur to us all without any apparent reason; mysteries that happen in all our potteries without any cause being discovered, and which disappear as mysteriously as they came.

Who is there here that has not run up against one of the following circumstances: A manager is called into the clay shops to see a quantity of ware cracked in the moulds, a very excessive amount, whereas the day before, and probably for months previous, the percentage of cracked ware is normal. He knows that he has got in no new material; all the clays, flint and feldspar have been taken from the same bins, and no error has been made in mixing. The next day, the trouble which existed practically with nearly all the clay, suddenly disappears and everything goes on as usual. What

has happened is probably this: The slip has been made too thin by the accidental running of too much water into the blunger. During the night the body has settled down in the bottom of the tanks into a hard mass, and if the slip pumps are started at the same time as the other machinery, nobody knows what is being pumped into the presses. It may be principally flint, or may be no flint, but whatever it is, it is not the body as originally mixed. The trouble does not end with your having some cracked ware in the clay shops. That which does not crack goes forward and fetches up in the glost warehouse as shivered or crazed.

It may be said that this is a very far-fetched case and not liable to happen, and if it did happen, can easily be remedied. In answer, let me ask: Is it remedied? Is it a common practice to start the agitators at least one hour before the pumps start?

Now the question is, will finer grinding help this difficulty? Most certainly it will, and the demonstrations I propose to make will convince the most skeptical.

I have here samples of two bodies made as under:

No. 1547—	American ground flint....	106
	Feldspar, regular ground..	42
	China clay..... ...	104
	Ball clay.....	59

No. 1548—	English ground flint.....	106
	Feldspar.....	42
	China clay....	104
	Ball clay.....	59

You will take particular notice of the fact that these two bodies were fired together in the same saggar and the series of cones 7, 8 and 9 were fired with them and that cone 7 is only slightly turned, and this is not a proper temperature at which sound biscuit earthenware can be made under our present system, yet you will find on comparison, that while body No. 1547 is very absorbent, No. 1548 is not nearly so much so. In fact, No. 1548 would pass as fair biscuit ware. Again, notice body No. 1547 is cut easily with the point of a knife, while No. 1548 will turn the edge of a knife, and lastly, as might be expected, there is a distinct differ-

ence in shrinkage between the two, No. 1548 being the smaller of the two.

These differences I ascribe to the fineness of grain to which the English flint has been ground. I had hoped to show in a very conclusive and positive manner by means of Schoene's elutriating apparatus exactly what this difference in grinding meant by making a series of determinations, and from their averages, give the quantity that passed over at the various current velocities indicated by the pietzometer readings.

Before giving you the results of my investigations by this instrument, I would like to state that it is comparatively new to me, and I hesitate somewhat in offering the determinations as evidence, because on the face of it I am in error, simply from the fact that I don't account for the 5 grains of flint with which I started in each washing test.

I will now give the result of my determinations:

	American Ground Flint.	English Ground Flint.
@ 1.25 Pietzometer,	.4576	.7378
@ 5.10 "	.4378	.4066
@ 17.50 "	.3027	1.8878
@ 60.00 "	2.3558	1.2034
Balance left,	.7298	.3050
	4.2837	4.5408
Decantation	.1326	.0889
Unaccounted for	.5837	.3705
	5.0000	5.0000

If I interpret these figures correctly, the showing is in favor of the English flint, on account of the great differences in regard to the quantity passing over at the lower velocities. The American ground flint showed nearly twice as much passing over at the 60 cm. pressure as the English flint did. The difficulty I had to encounter in these investigations was this: That although I followed the best instructions I had as to the method of preparing the material, I was unable to get a clear liquid at the lowest reading, viz: 1.25 cm. In some of the trials I made, I allowed the mixture of the flint and water to stand two days, and after that time there

was a distinct milkiness, which no filter paper (at least in my hands) would arrest.

I mention this fact, which was identical in both varieties of flint, because I feel very sure that the items "Decantation" and "Unaccounted for" belong to the 1.25 cm. reading. I will further explain that the item "Decantation" was that portion of the liquid that still appeared as a cloudy fluid and *was arrested by a filter paper* and was weighed.

In short, if the figures of the test are rearranged as I think they really belong, we should have:

Description of Sediment.	English Flint. Percents.	American Flint. Percents.
Finest sizes, lost in process.....	7.41	11.67
Too fine to settle, but was caught on filter paper.....	1.78	2.65
At 1.25 cm. Pietzometer, slowest measured speed. Particles immensely fine.....	14.75	9.15
At 5.1 cm. Pietzometer; still exceedingly fine.....	8.13	8.76
At 17.50 cm. Pietzometer; still very fine	37.75	6.05
At 60 cm. Pietzometer. Particles relatively coarse, though still not to be distinguished clearly by the unaided eye.....	24.07	47.12
Coarsest particles, left after all others had floated away. Grains clearly visible to the eye.....	6.10	14.59
Totals.....	99.99	99.99
Total sediments down to and including the 17.50. cm. sediment.....	69.82	38.28
Total sediments, coarser than the 17.50 cm. sediment.....	30.17	61.71

In connection with the grinding of materials, I have so far only touched on the infusible portion, viz., the flint, but I now want to call your attention to quite as important a point, viz., the feldspar.

Some years ago, I made the experiment I have here repeated—and offer for inspection—but the marked difference in fusion as it is affected by the grinding is not nearly so

pronounced in this experiment as the one made sometime ago, because the experiment was not made under the same conditions.

The experiment consisted in grinding a batch of feldspar in a pan and removing from the pan at the end of each hour a portion of the feldspar slip. This slip was hardened on plaster and made up into cones, similar in size and shape to the Seger cones. These cones were then placed on a slab of white ware body, together with Seger cones Nos. 7, 8 and 9, and were fired in a regular biscuit burn. The feldspar taken direct from the bin gave a cone which stood up throughout the test without fusing. The cone representing one hour's grinding showed a distinct improvement in fusibility, and so on with each hour's grinding until the fourth, after which additional grinding seemed to produce but little effect. In the first experiment I made, I ground the trial on a pan having an area of 28 square feet, and this was made on a small color pan of 1¾ feet area, and on account of the difference between the weights of the runners in the respective pans, the degrees of fusion were not so clearly shown in one case as the other. Still, it will be seen that the sample marked "O," which is ordinary feldspar, taken from the bins, is very different from that marked 1, 2 and 3, but after it passes the fourth hour the change is apparently imperceptible, even up to the eight hours' grinding. On the rear in second row of cones on the trial slab you will notice cones 7, 8 and 9, cone 7 being down

Having stated my case, let us see whether anything practical can come from this paper. Is it of sufficient importance for this society to appoint a committee to devise a method for testing flint and feldspar? I refer, of course, to a mechanical test, which we might call the "American Ceramic Society's" test? Let us first of all know what we want, and then when we buy material, ask the seller if he will guarantee it to stand the society's test.

One half of all the earthenware we turn out is composed of flint and feldspar, and sometimes we hear of a certain firm of flint and feldspar grinders who grind very fine, but in nine cases out of ten the only question that is asked is will

it pass a 12*s* lawn well. Because it passes a 12*s* lawn, does not necessarily fit it for an earthenware body.

We may, if the Society desires to go into the question at all, make it a lawn test if that is the best, but, if they do, make it fine enough and say that a certain per cent. must pass the standard lawn they adopt.

To grind finer means more expense, but that need not prohibit its use. Suppose we do make a test, and the flint grinder says I will grind it as you wish but it will cost you so much more, then it is up to the user to say whether he wants it or not.

If any move is made in this direction, it must be made by the ceramic chemist, for it is eminently in his province.

DISCUSSION.

Mr. Stanley G. Burt: Mr. Mayer treats there a subject which is a hobby of mine. I have felt all along that the subject of grinding was of greatest importance; that the fineness of grain was the great keynote to many troubles, and many will remember that, as stated in my paper, that my results were equivalent to his. I did not go as far as he goes in making the suggestion of a committee to investigate the subject and set up a standard. I think the idea is very good; but in all my conversations with grinders, they say it is absolutely impossible to do that, because the additional cost necessary to get additional fineness of grind will not be stood by the American consumer. I could not give the figures, but flint furnished to pass a No. 14 lawn would cost considerable more than double if put into condition to pass a No. 16 lawn. It would mean considerable more than double the work and would add considerably to the cost. I think anything which might be accomplished in that line would be a great gain. While we may not be able to set up a standard, to say that it shall pass a certain lawn, we could insist on uniformity. I think it would be a good thing for the potters to agree among themselves upon a standard, say to pass a No. 14 lawn, then insist to the dealers that shipments must pass that lawn. I have found a number in my experience that would not begin to pass a No. 14 lawn. It seems to me

feasible to adopt a minimum standard, but I doubt if we could push it to a No. 16. But we could insist on the material passing a No. 14 lawn. I think this is one of the most important matters — fineness of grain — but I do not know whether there should be more committee work.

Mr. W. D. Gates: I move that a committee be appointed, of which Mr. Mayer shall be chairman, and Mr. Burt and Professor Orton other members, to take this matter in hand and make report.

I think we will always find that the buyer is the "doctor." If he insists on a thing, the people will "get busy" and get it, if his demands are anywhere within the range of possibility. We occasionally run across people ourselves who want things which we say at first are impossible, but if they insist on them, we have to go to work and try to make them. If fine grinding is done in England, we can certainly do it in this country.

Professor Orton, Jr.: I approve of the idea of this society trying to define something like a standard of fineness. I think it is an excellent idea. Whether we can enforce what we think is right and proper in the matter is another question altogether. I think what we do accomplish in that direction will be more in the way of giving each buyer arguments with which to hold up his end of the controversy, when it comes to dealing with the spar miller or flint miller, than in any other way. This society has no standing in the commercial world, and no lever by which it can bring into line either the spar or flint miller. Both are influenced by commercial considerations. It seems to me, therefore, that our attitude can be simply advisory. We set up the facts, but it be the buyer who enforces our findings with the spar and flint millers.

I am exceedingly pleased with the facts which Mr. Mayer has brought out. I have had a little experience with that Schœne apparatus myself, and if Mr. Mayer was here I could reassure him as to the quality of the results, which he feels so uncertain in presenting. The apparatus is not a perfect one, and I never yet saw a man who could take five grams of material, wash it into a number of grades of

fineness with the Schœne apparatus, and get back all with which he started. It seems physically impossible to save all the material; and I think Mr. Mayer has made, on the whole, a very creditable showing in his first attempt, and his results are similar to what I have myself secured and seen others get. He has proved his point by the use of this apparatus. There is no kind of question about that. The respective valuations of this one line (indicating on the chart) "2.3558 against 1.2034" show that. The American flint shows twice as much as the other. The whole experiment shows the great difference in physical characteristics. It seems to me that what a committee appointed by this society should do, is not to say what the spar miller should produce, or what the dealer should buy, but what the size of grain should be in order to accomplish certain results. Then let the commercial question settle itself.

For instance, Mr. Mayer says that beyond a certain limit there is no use for further grinding. Wouldn't it be well for this proposed committee to say how fine it is necessary to have material ground in order to produce certain results? Mr. Bleininger read a paper last year, touching on cement manufacture, in which he gave carefully prepared data on the sizes of grain of flint and spar. He illustrated the effect which the size of grain had on the decomposition of the material by lime, and clearly showed that either flint or spar above a certain size was well-nigh useless in cement, being simply inert material at the close of the process. But below a certain size it was chemically valuable, because, under standard conditions, it would be converted into soluble matter, and form a part of the cement. It was very clearly shown in that problem what the limiting conditions in cement work are. It seems to me, if we can do the same for the use of flint and spar and other insoluble fluxes in use in this country, and show that good results can be gotten from flint and spar of certain sizes, and recommend that they be considered as standard, then the whole situation will be cleared up, and anybody who desires to avail himself of our information or our work, can do so. It seems to me this is the only basis on which we can stand.

In regard to the constitution of the committee, I believe I was named as one, but I suggest that this would be a bad appointment, because I am not a great user of such materials and have little influence commercially. There are others here, who purchase thousands of dollars' worth a year, and other things being equal, they would have more influence.

Mr. Edward C. Stover: In looking over Dr. Seger's work on the question of grinding, find he touches that point, in giving the methods of changing the coefficient of expansion. In setting forth the ways of making the change, among other things, he says to grind the flint or spar finer. I took the matter up with a local spar grinder, and asked what the extra expense would be of making a finer grind. He said he had experimented along that line, and if I remember, he said that they ground an ordinary charge in a common ball mill, three or four hours longer, after the time when they considered it ready to deliver to the potter. He said in dry grinding, the difference in fineness of the material was so slight after double time, that to get such results as I wanted, would make the expense greatly more than any potter would pay. That came into my mind when Mr. Mayer's paper was read. I presume his American flint was dry ground, and the English flint wet ground. In short, he is comparing dry grinding with wet grinding, As I understand it, English material is delivered in the slop state, while here we get it dry.

Professor Charles F. Binns: I am very much struck with the calm way in which this work is handed over to a committee, without much idea of the immensity of the problems involved. It would probably take two or three investigators a year's time to standardize tests, in order to find out what they were really driving at.

The state of affairs in England is that the bulk of the larger producers grind their own material. Every large factory, I may say, without exception, has its own mill. The smaller ones buy the material ready ground in the slop state for the most part. This involves wet mixing instead of dry, which is not in accordance with the American idea of speed. The manufacturer cannot get out anything like as many tons in the same time by the wet process.

In England the materials are prepared and lawned in the slop state. The lawns are sixteen and eighteen for common wares, and each material is lawned before mixing. In some cases ball-clay is put through three decks of lawns. Under such conditions you would have to reconstruct every plant in the country and you will not find one potter in ten who would consider the matter even if you put to him the inducement that he can make better ware at a lower cost. You ask the Belleek manufacturer what is the secret of success in the manufacture of Belleek, and he will say that it is fine grinding. The material is ground until it is an impalpable powder; when tested between the thumb nails there is no grit. In a factory I was once connected with, there was a time when we kept a man all the time chipping spar, rejecting the brown parts of the spar. The question of grinding was raised and we made a test. A bucket full of the chips were put into the ball mill and ground perhaps six hours, and the Belleek made from it was every bit as good as that made from the selected spar, showing that the whole problem lay in the grinding.

I do not believe it is possible, with the present equipment of our flint mills, to give finer material. The cylinders are wood-lined, and the flint and spar are ground dry. If some miller had the courage to line his cylinders with porcelain or "French burr," and to grind wet, filter-pressing the slip and drying the product to within 5 per cent. of absolute dryness, he would have the pick of the market. Wet ground flint can be lawned, dry ground cannot, and this alone is a strong argument in favor of the wet method.

But, as a society, in the face of the fact that all the millers are grinding dry and all the manufacturers are mixing dry, we cannot, I fear, recommend any such standard as that which obtains in England. Unless we give the committee some definite instructions or prepare the way by preliminary work, it will not do much good.

Mr. F. W. Walker: The remarks of Prof. Binns are very much to the point and I think all of us, possibly, are familiar with these conditions. The grinders of flint and spar will not voluntarily grind finer. I had quite a talk with

one of them a few weeks ago, and he said they could not do it. I had just been talking with Mr. Mayer, and he said that to grind as fine as Mr. Mayer wanted it, would make the material cost double the money. He said, "Would you pay it?" I was not prepared to say whether I would or not. I was simply advocating along the lines of Mr. Mayer's paper. I think the subject wants to be thoroughly considered, but I do not know any one better able to consider it, and make report, than the committee suggested.

SUBSEQUENT COMMUNICATION FROM MR. ERNEST MAYER.

Although not present at the meeting, I am very glad to have the chance of seeing the stenographer's notes of the account of the discussion on my paper.

I fully expected, in fact anticipated, the criticism made by several that the grinders of flint and feldspar would protest against any change in the present method of grinding. My answer to that is this: That as a matter of fact the grinders of flint and feldspar don't know how coarse or fine they are grinding, and my experience is that they grind material as short a time as they possibly can, and the only limit they admit is when the buyer gets a car load of material that won't go through his lawns and won't pay for it. Then, and only then, will they grind finer, unless it so happens (as has many times been done) the potter will charge his lawn to suit the coarser flint.

My paper was written as a protest against the haphazard method of the grinding now in vogue, and, while the point was omitted in my paper, I fully intended to bring out the fact of the great irregularity in grinding in the different cars delivered.

This whole matter is a very simple business proposition and resolves itself into the following: The flint grinder has a grade of flint to sell at a certain price which is ground a certain number of hours, which we will call his *regular flint.* Now for every hour's grinding after that point a fixed sum should be charged, and it will then be up to the manufacturer to decide what he wants.

A committee appointed by this association could ascertain exactly what the finer grinding meant by making pieces of ware from flint and feldspar ground in the different number of hours, and if the results warranted it, after the results of the experiments had been laid before the association, each member would would take advantage of the information gained and govern himself accordingly.

I am not foolish enough to think that my paper will be the cause of revolutionizing the flint and feldspar grinding business, but if the remarks I have made carry any weight, by the arguments used to substantiate them, then my paper has accomplished its mission, viz.: that finer ground material than we now get is one of the means of making more reliable and better ware.

There is one matter to which I would like to call the association's attention, and that is, what is a No. 12*s* lawn? As a matter of fact we come to these meetings and talking about glaze or body being sifted through a No. 12 or No. 14 lawn, it appears to me that no two of us are talking about the same thing. As an example: Professor Binns in discussing my paper says, "In England the materials are prepared and lawned in the slop state, the lawns are 16 and 18 for common ware." Now, evidently, we are not talking about the same thing, because if the common ware goes through an 18*s* lawn, what kind of a lawn do they use for the best ware? Except for stain, I never heard of a lawn being used either here or in England finer than 16.

My understanding of a 12*s* lawn is that it means 30 meshes to the ¼ inch, or 120 meshes to the inch; a 14 lawn 35 meshes to the ¼ inch = 140 meshes to the inch, and a 16*s* lawn = 40 meshes to the ¼ inch = 160 meshes to the inch; but I think the question of the names and number of meshes to the inch ought to be settled and to arrive at an understanding of what the various sizes actually mean in count.

CRYSTALLIZED GLAZES.

BY

WILLIAM H. ZIMMER, PH. D., COBURG, GERMANY.

During the first three years of the existence of this Society we have added quite a good many valuable contributions to the knowledge of silicates. Silicic acid in its various forms has occupied our attention more than anything else, and all papers tend to demonstrate the importance of fineness and most complete distribution. When older methods stopped, after showing that a certain amount of silicate matter was present, we went one step further in trying to decide, if it was qualitatively suitable to the purpose as well as quantitatively. Our transactions contain most important researches in regard to tensile strength of clay-mixtures, plasticity and the relation between plastic clays and the non-plastic ingredients of ceramic bodies. Glazes and fluxes have had their share in our discussions, and even the origin of kaolin, and the nomenclature of the silicates has been touched upon, not to mention all the papers treating on the subject of firing kilns, and similar topics, in which the manufacturer of brick is much interested as the manufacturer of fine china.

Yet, to be frank, there seems to be a large deficiency in the knowledge of our ceramic bodies themselves, and I almost believe that the slow progress in establishing uniform rules and tests in ceramics is partly caused by insufficient knowledge of the very foundation of our ceramic art. What are our bodies and glazes? In some of them, baked at a very low temperature, the various components have hardly entered into a chemical action and maintain as finished ware more or less the nature of a mechanical mixture. Others become quite uniform, and seem to undergo such an intense change, that the final product appears to be almost a chemical compound. However, the chemists claim that a chemical

compound cannot be still considered as a union of various molecules; but where can we draw the line between a chemical compound and a union of various molecules in our ceramic products? May we consider the translucent hard porcelain as a well defined compound, or is it not rather still a union of molecules, of substances partly dissolved, partly suspended, in the melted feldspar? As Prof. Orton said in one of his previous papers,* we have been working for years with test tubes and balance, seeking to explain the marvelous differences and similitudes, but we have decidedly neglected the use of the microscope, and I would like to urge the members of our society to become microscopists and to open up new methods, which will not only allow us quick investigation, but will very likely lead to a more thorough knowledge of ceramic bodies and materials. Such methods might also give us a chance to discover in our clays and kaolins a variety of the rarest elements, which we could not detect with our slow chemical analysis, but which undoubtedly will have certain influences.

There might be a way of crystallographic analysis. We all know, that in chemistry we crystalize a substance repeatedly in order to purify it. The process of crystallization is of the utmost importance, and we shall advance more rapidly in ceramic theory, if we devote more attention to crystals and crystallization. In kaolin, as well as generally in ceramic bodies, the crystals are very fine grained, but of late glazes with splendid crystallizations have occupied the attention of the ceramic world, and although those articles with crystallized glazes are too difficult to be manufactured on a large scale and too high priced to become a staple article, they are very interesting from a theoretical point of view. They are, at least, a start in a new direction and I beg permission to occupy some of your time in describing the methods of producing them.

Our glazes are more or less perfect glasses, in very thin layers and may be considered as solutions of silicates, which although not uncrystallizable, do not flash readily into the crystalline state. However, those glassy silicates form

*Trans. A. C. S., Vol. II, page 100.

definite crystalline compounds, if given a long time to cool. Actually, the crystalline or glassy conditions are more or less accidents of cooling. Crystalline segregations are not new in colored glazes. Aventurine effects are found on Japanese vases made centuries ago, and I have now in my possession a Japanese vase, showing tiger-eye effects on some parts, which was certainly not made within the last twenty years, even if I do not venture to determine its true age.

Dr. Maeckler, by request of Director Dr. Heinecke, of the Royal factory at Berlin, who had purchased some Rookwood vases at the World's Fair, 1893, for the museum of the factory, made the first scientific investigation of the phenomenon, which was published in 1896. Aventurine glazes have been made by various factories in the United States and in Europe. I recently received a nice collection from Egersund, Norway, but I do not know of any one who has reached the perfection of the crystalline glazes of the Rookwood pottery. However, the technique of crystallized glazes was not further developed, until at exhibitions at Paris and Brussels and also at Stockholm in 1897, the French National factory of Sevres, the Royal factory at Copenhagen, and Rohrstrands Aktiebolag, of Stockholm, came out, almost at the same time, with artistic vases covered with crystallized glazes, while the firm of A. Bigaud & Co., in Paris, was successful in the same direction on stoneware. In 1898 the Royal manufactory, of Berlin, followed with some splendid specimens, and of late quite a few factories have been successful in producing crystallized glazes, and although these art goods cannot be produced by the thousands, the time has passed when the formation of crystalline glazes could be looked upon as a mere accident.

It seems as if the very first study on that subject was made by Ebelmen as early as 1847 – 1852, but the credit as inventors of those crystallized glazes is due to Charles Lauth and G. Dutailly, who furnished a cup and paper on the subject for the museum of the National factory at Sevres on September 19th, 1885. An interesting article, "Sur les Couverts de Porcelaine," by Chas. Lauth and

G. Dutailly, is to be found in the book: "La manufacture nationale de Sevres, 1879–1887," par Charles Lauth, in which they say:

By introducing oxide of zinc into glazes, which increases only slightly the fusibility of the glaze, it is possible to obtain highly basic glazes, which form during the process of cooling small crystals of silicates or more likely of oxide. The best crystals were obtained in a glaze of the following mixture, melting at 1350 deg. Cels.

Pegmatite	55.60%
Sand	16.00
Kaolin	4.40
Lime	12.00
Zinc Oxide	18.00
	106.00

This would correspond to the following composition of the glaze:

SiO_2	57.49%
Al_2O_3	11.68
CaO	6.72
Na KO	6.12
ZnO	18.00
	100.01

In another short paper, read before the International Congress for Applied Chemistry at Vienna, 1898, Clement of Copenhagen describes his experiments with the silicate of zinc on the hard porcelain of the Royal factory, Copenhagen. On the porcelain, which was first heavily glazed with the regular glaze and fired as usual, he applied with a brush a second heavy coat of $SiO_2 + ZnO$. After the second glost fire the glaze appeared to be saturated with small crystals, which he claims to be crystals of the natural silicate zinc, Willemite, Zn_2SiO_4. In his glazes, which must be burnt in oxidizing fire, the coloring oxides of metals gives the following effects: Oxide of copper celadon-green, oxide of cobalt, blue, chromium and tin together a yellow-red and the oxide of nickel, which would generally cause a brown color, gives in combination with the silicate of zinc a Persian blue glaze with blue crystals.

A more recent publication of G. Vogt, technical manager of the National factory at Sevres, says that the first crystallized glazes were made at Sevres in 1884. Since then the process has been greatly improved and is applied with good chances of success to stoneware as well as to porcelain. It has been found of great advantage to fritt all materials for the second or crystallizing glaze-coat in a strictly oxidizing fire, for which fritting crucibles are used. He gives the composition of two fritts in practical use at Sevres as follows:

	I	II
Pearl ash (K_2CO_3)	138	69
Zinc Oxide	162	202½
Flint	360	350

Both fritts were mixed; best result was obtained by mixing 35 parts No. I and 15 parts No. II.

A yellow crystallized glaze was obtained by fritting

	III
Pearl ashes	138
Zinc Oxide	162
Flint	300
Rutile	82

15 parts fritt No. II and 85 parts fritt No. III are mixed to good advantage. Rutile contains a good many impurities, principally ferric oxide, and this seems to be the coloring agent, while chemically pure titanic acid will give pretty colorless crystals.

Summing up all these publications, there seemed to be no doubt, but that the silicate of zinc was the essential substance, which, being dissolved in the melted, liquid glaze, would separate again in slow cooling, and I concluded by following this general idea, it would be impossible not to obtain crystals. I proceeded, therefore, to fritt in equivalent proportions pure zinc oxide and silica, using a 99 per cent. dry ground flint. Then I prepared a thick slip by mixing the finely ground frit with water and applied a heavy coat on various small articles of our Wheeling pottery fancy goods. They had been through the glost-kiln before, and for this second glost-fire, in order to obtain crystals, I placed them in a covered pot, put the latter into a large sagger, which I filled solidly with sand, placed the whole in the

middle of the second ring, well protected from any possible draft after the large kiln would be open, and felt satisfied that I had done everything in my power to procure a very slow cooling off. But the result was anything but gratifying. The very best that I could show for all the trouble were some white formations like moss, which proved to be something like small crystals under the magnifying glass, but which were quite different from the splendid specimens I had read about.

I had no chance to investigate any further, but of late my observations were confirmed by the experiments of an Austrian ceramist, who published his results under the anonyme "Bries" in No. 2 of the Sprechsaal, 1902.

He started from a china glaze of the formula

$$\left.\begin{matrix}0.4\ K_2O\\0.6\ CaO\end{matrix}\right\}\ 0.5\ Al_2O_3\ \ 4.5\ SiO_2$$

and increased the basic side by adding one equivalent ZnO.

The new glaze which had to be fritted, would correspond to the formula

$$\left.\begin{matrix}0.2\ K_2O\\0.3\ CaO\\0.5\ ZnO\end{matrix}\right\}0.25\ Al_2O_3\ \ 2.25\ SiO_2$$

The body he was experimenting with had the following composition:

Clay substance	35%
Flint	40
Spar	25

The trials were fired at a temperature of Seger cone 8 that is at a heat quite frequently reached in our American biscuit-kilns. But neither the glaze mentioned above, nor a plain potash zinc bisilicate

$$\left.\begin{matrix}0.3\ K_2O\\0.7\ ZnO\end{matrix}\right\}\ 2\ SiO_2$$

gave the desired result. Then he continued: "I did not succeed in obtaining gratifying results as long as I restricted my experiments to glazes without titanic acid. There was only a tendency to small formations, which sometimes could be proved to be crystals under the microscope. In consequence I went one step further and fritted another glaze, in

which part of the silicic acid was replaced by an equivalent amount of titanic acid,

$$\left.\begin{matrix} 0.8\ K_2O \\ 0.7\ ZnO \end{matrix}\right\} \quad \left\{\begin{matrix} 1.7\ SiO_2 \\ 0.3\ TiO_2 \end{matrix}\right.$$

obtained by the following mixtures:

Pearl Ash	18.43%
Zinc Oxide	25.24
Flint	45.89
Rutile	10.94

"The glaze became opaque. Titanic acid does not go into a complete solution in the liquid glaze, its molecules remain more or less suspended and this circumstance seems to be very material for the formation of large crystals. Besides a hard biscuit, a long period of an evenly high temperature at the end of the burning and, as mentioned before, a slow cooling are the most favorable conditions." He also recommends to burn the crystallizing glazes in double saggers and calls attention to the necessity to have the inside of the sagger well glazed, as a porous sagger would absorb alkalies from the glaze; which might be detrimental to the crystals. Another interesting item is the fact, that in colored crystallizing glazes the crystals sometimes have taken up the coloring oxides and appear in their color, while the ground shows a different shading.

The effects obtained by this application of titanic acid are really wonderful and I exceedingly regret, that I cannot send along some of the fine specimens standing before me while writing.

DISCUSSION.

Mr. Hasburg: I have a small specimen of the Copenhagen crystalline glaze with me which I will pass around if the members care to see what this paper refers to.

Mr. W. D. Gates: One thing which has struck me is the use of the microscope in the study of crystalline glazes. I was much interested in having it pointed out to me that we we manufacturing a matt glaze which was a mass of crystals. Until Mr. Burt called my attention to it, I did not know what we were making.

Mr. Ray T. Stull: I was interested in Dr. Zimmer's paper, as I happened recently to find in Sprechsaal the same article which he speaks of, about crystalline glazes. I immediately became very much interested. I weighed up a small batch of glaze and fritted it, and applied it to a hard porcelain body which burned very dense at cone 8. The factors which go to produce a crystalline effect were given in this article as, first, a continuously oxidizing fire; second, a gradually increasing temperature; third, a gradually decreasing temperature; and fourth, the elimination of the alumina. The composition of this glaze was

$$\left.\begin{matrix} 0.8\ K_2O \\ 0.7\ ZnO \end{matrix}\right\} \begin{matrix} 1.7\ SiO_2 \\ 0.8\ TiO_2 \end{matrix}$$

This glaze is free from alumina, and when fused, lies quietly. I think the paper said the titantic acid did not go into solution. In the fritt I made, I found it did. After making the fritt, I applied the same in a dab in the center of a small tile. I did not have access to a kiln having the proper temperature at that time, and so I put it back into the fritt kiln, where it is almost impossible to maintain oxidizing conditions, or to raise the temperature slowly, or to cool slowly. However, I did the best I could with these unfavorable conditions. On examining the results, I found the center was perfectly clear, but around the rim of the glaze were these little flossy, silky crystals, which we saw in the piece which Mr. Hasburg passed around.

Professor Binns: In regard to the production of crystalline glazes, it seems to me that it must always rank with the Rookwood Tiger Eye or Gold Stone, as more or less of a ceramic curiosity. It must not be supposed that it will be of commercial value. It ranks with the fine art productions which made the old Chinese so famous. Such productions are fine art and not science. I am coming more and more to the conclusion that the production of fine ceramics is as much a matter of art as of science. This is illustrated by a conversation which I had with a Japanese student in my school regarding the production of copper reds. He says that the most successful man in Japan in the production of copper reds told him that about one piece out of two hundred is

successful, and moreover he finds it a mistake to use artificial material if he can get the natural. He won't use ground silica in any form. He gathers together a mass of rice husks and calcines them to procure finely divided silica from the ash. This opens the limitless field of research (if it was limited before, which I doubt). The Japanese potter who picks up a stone by the roadside and uses it obtains a result which he cannot duplicate, but he may get a fine thing. The production of crystalline effects belongs to the same class of beautiful and rare ceramics, of which we are beginning to take cognizance. We are coming to see that there is more than cold science to be reckoned with. It is the art which make a physician a good physician, and the art which makes a potter a good potter. It is the art which has made some practical men superior to some men depending only on science, and which we always have to take into account in the production of finer work.

Mr. W. D. Gates: Regarding that Japanese silica derived from rice husks: doesn't that produce an intensely fine silica?

Professor Binns: That was the point in glazes. This intimate mixture of silica and carbon, which they depend on to help the reduction of copper to the red color, was secured from rice husks.

Mr. J. W. Hasburg: Concerning copper reds being so accidental as one in two hundred,—that might be the case with the man who did not know his business and does not know how to get the copper red. I do believe it is a fact from a scientific standpoint, that it is a purely chemical process. It is produced by special conditions and chemicals, and when the conditions and chemicals are right, the red will be produced. I believe that where the production of copper red is thoroughly understood, it is not a case of one chance in two hundred, but that it will be produced nearly every time with little variation. I have succeeded in producing these reds and, therefore, can speak from experience.

Professor Binns: Just as I said before—the production of copper red is not under the control, absolutely, of any known set of conditions. Of course there are special conditions to produce it, but at the present time we do not know

exactly what they are. We could hardly find a higher exponent of science than Seger; yet his copper red is not to be compared with that produced by the famous Lang Yao family in China.

Mr. W. D. Gates: My experience with copper red is that you want to get your theories in good shape, go to work and flirt with it — then do something else.

Professor Binns: It is a matter of common knowledge that one of the Havilands of Limoges spent 150,000 francs in an effort to produce copper reds, and after some years consumed in experimenting, gave it up as a failure. Looking at some of his samples in the light of present knowledge, I think I can see where he failed, but I don't believe I could do any better.

STONEWARE SLIPS OR ENGOBES.

BY

SAMUEL GEIJSBEEK, DENVER, COL.

Engobes or slips are natural clays or clay mixtures designed to cover other clay bodies, whose color we desire to conceal. They also can serve as ground-layers for decorative effects, in case the article is to be decorated. Generally an engobe is used to increase the value of the product on which it is applied. In these cases, the engobe is a more expensive mixture than the clay body on which it is used. Sometimes its value is the same as the clay on which it is applied, and only serves to cover the color.

The use of engobes is a very old one, and history tells us that the Greeks already used engobes in three colors—white, red and yellow. Edwin A. Barber, in his book, "Pottery and Porcelain of the United States," says in regard to engobes: "Perhaps there are no products of the potter's art more interesting to the antiquary and the collector than the rude 'slip-decorated' pieces which were made in England and Germany during the seventeenth and eighteenth centuries."

Engobed ware was made in Pennsylvania as early as 1762 and the industry flourished until about 1850. Engobed ware was not made thereafter in large quantities until the art potters took it up some years ago. Stoneware manufacturers are now using it more and more, and we find quite a nice variety of "white lined" clay products throughout the country.

In the art branch of ceramics, engobes are extensively used. These engobes are compounded in order to obtain decorative effects and are commonly termed "colored slips." Terra-cotta manufacturers are using engobes to a very large degree, and produce some very good effects with the same.

The nature of engobes can be twofold. They can be white or colored. In the first instance, they are mixtures of

pure white burning clay materials, and in the second way, they can be natural colored clays or mixtures of clay, tinted with color-producing mineral ingredients.

On stoneware, the engobe is usually white, while on artware and terra-cotta, mostly colored.

Engobes are mostly applied to the ware in the green state, either by dipping or spraying. Experiments have been made to apply the engobe on the dry ware, with various success. It is evident that both ways can be used, and it depends upon the product on which the engobe is to be applied.

Engobes can be covered with a glaze, or they may themselves form a vitrified coating. When an engobe is covered with a glaze, it can be applied to the engobe in green state, or after the engobe is burnt once. To the first class belong stoneware, and to the other class, artware.

In Germany we find a few clays which are put on the market and are specially adapted for engobe purposes. We do not have any special grade of this kind of clay on the market in this country, as clay-miners never have made any effort to foster that trade.

In order to make a clay valuable for white engobe purposes, it should possess a certain degree of plasticity; it should burn white; it should be able to adhere to another clay in such a way as to give a uniform coat when applied as a thin slip; it should have the same shrinkage as the clay on which it is applied, and should be able to hold a glaze without defects.

According to Dr. Seger, the two German engobe clays which are on the market have the following composition:

	Kaschkau Clay	Cologne—Meissen Clay
SiO_2	68.17	62.52
Al_2O_3	25.09	25.57
Fe_2O_3	0.64	0.92
CaO	0.85	0.65
MgO	0.26	0.10
K_2O	0.80	1.04
H_2O	9.70	9.27
	100.01	100.07

Rational analysis of both:

Clay substance.	67.82	72.05
Feldspar.....	1.25	0.10
Flint.....	30.93	27.76

I have so far been unable to find in America a white burning plastic clay which would answer for engobe purposes, and which would have nearly the same composition as the German Clays.

The clay mined and marketed by Fischer, Black & Co., of Newport, Pa., is the only clay of which I know which comes close to the German engobe clays cited above. The analysis is given below:

	Analysed by Langenbeck	Analysed by Geijsbeek
SiO_2.	62.89	62.30
Al_2O_3	26.14	27.07
Fe_2O_3	0.57	0.68
CaO	0.60	0.64
MgO.....	0.75	0.18
K_2O	2.32	1.85
H_2O.....	7.55	7.74
	100.82	100.36

Rational analysis:

Clay substance.	70.81	68.70
Feldspar.....	2.17	2.42
Flint.....	27.84	29.24

I have made some thirty different experiments with this clay for engobe purposes without any satisfactory results, and have come to the conclusions that this clay does not answer for that purpose, although it is in chemical composition nearly like the German clays.

In making further experiments along this line, I used various American kaolins as they are put on the market, and of the numerous varieties tested, the Florida kaolin and the Graham kaolin answered the purpose best. I first used the Spencer kaolin, with various success, but abandoned the same later on for commercial reasons.

In conducting such experiments, there are at present no rules which could be used in guiding one in order to obtain satisfactory results, and I am sorry to state that so far, my experiments have not yet given me a basis from which to formulate a rule which would be useful in making further experiments.

It is, strictly speaking, an experiment in which plasticity and shrinkage play the greatest part.

I have found that in making colored engobes it is much easier to obtain a satisfactory clay than in making white engobes. For such purposes I have been able to use regularly the ball clays without any additions. As coloring material, I have found that the ordinary underglaze colors on the market answer the purpose very well.

In making engobes for stoneware, it was always desired to have a white engobe, which could be used for inside work, such as for jars, milkpans, buttercrocks, churns, and so on. Stoneware people call these engobes under the familiar name of "white linings or enamels."

My first experiments were conducted on washed stoneware clay as used by Sherwood Bros., New Brighton, Pa.

The rational analysis of this clay is:

Clay substance	66.54
Feldspar	2.46
Flint	31 02

I determined first the relative shrinkage between the Florida kaolin and the stoneware clay, as I intended to use the Florida kaolin as a basis for the experiment.

A thin coat of kaolin slip on the stoneware would be shivered off entirely, after burning, showing that the kaolin shrunk more than the stoneware clay.

The addition of non-plastic material was next tried. 75 per cent. of kaolin, and alternately 25 per cent. of flint, feldspar and whiting. Of these three experiments, the two last showed the best results, and further experiments were made in that direction.

Engobe Number	B_3	B_4	B_4	B_6	B_8	B_7	B_8	B_{19}	B_{21}	B_{20}	B_{22}	B_{24}
Florida kaolin	75	75	75	75	75	75	75	70	70	65	65	65
Brandywine Feldspar	25	..	20	15	12.5	10	5	10	15	10	25	30
Whiting	..	25	5	10	12.5	15	20	20	15	25	10	5

Of the above twelve experiments it was found that B_7 answered best for stoneware slip, and next best was B_{24}. The difference between the two was that B_7 was whiter than B_{24}. None of these mixtures peeled on the stoneware clay, the only defects they showed was cracking in drying on a few pieces. The only trouble with B_7 was that the shrinkage was just a little less than the body, so that when applied on the outside of the ware it would crack off the edges of the body. Therefore it was advisable to see what another kaolin would do. Spencer kaolin was substituted for the Florida kaolin. This substitution obviated the trouble above mentioned at once, and the mixture gave very satisfactory results,

While engobe B_7 did not shrink enough, and none of the other experiments showed any improvement in that respect for outside work, I concluded that the cause was due to too much plastic clay. I calcined some of the Florida kaolin, and made mixtures with the same formulæ B_7, but varying proportion of calcined kaolin. I took account of the loss of weight in the calcined kaolin, in weighing it into the new test batches, and obtained an excellent engobe for outside work. B_{29} gave the best results.

Engobe Number	B_{27}	B_{28}	B_{29}
Florida Kaolin, Raw	60	50	35
Florida Kaolin, Calcined	15	21.5	34.5
Brandywine Feldspar	10	10	10
Commercial Whiting	15	15	15

These few experiments show that there are several ways to get a suitable "white lining" or engobe for stoneware, and judging from past experience with the above engobes, I think that B_{29}, where calcined clay was used, would give in the long run the best results, especially on stoneware, where

body, engobe and glaze are all brought to perfection in one burn.

All samples were fired at cone 8, and the glaze employed for all engobes was

$$\left.\begin{matrix}0.8\ K_2O \\ 0.7\ CaO\end{matrix}\right\}\ 0.5\ Al_2O_3\ \ 4SiO_2$$

In connection with these experiments, I beg to call your attention to the solubility of bodies and engobes in glazes. When I started these experiments, I was told to use the regular white stoneware glaze employed by Sherwood Bros. at that time. I judge it was a zinc-boro-lime glaze, but do not know its composition. On using this glaze, all engobes showed that the layer of white material was attacked to such an extent, that the body was plainly visible, and the effect of the engobe was lost.

Engobe glazes should be so compounded that their solvent power is very low. Easily fusible glazes, which do not need a high fire to be called "finished," will not dissolve any important part of the body, if they are not fired higher than their proper finishing point. But when fired at a high heat, their solvent action on saturating themselves with the ingredients of the body, is very great, and such glazes can not be used for engobe purposes. Glazes, as generally used in stoneware potteries, high in alumina and silicic acid are usually well adapted for engobe purposes.

Some time ago I was called upon to make engobes suitable for western stoneware clays. I soon found that engobes which work well on one clay, may not work so well on another.

The clays in questions were the Colchester, Ill., and Tennessee, Ills., stoneware clays as used in Macomb and Monmouth, Ills.

The rational analysis of these clays is as follows:

	Colchester	Tennessee
Clay substance ...	54.60	65.70
Feldspar.....	4.51	2.25
Flint.....	40.89	32.05

The Tennessee clay resembles very closely the New Brighton clay in rational analysis, and works about as well.

Engobes B_7 and B_{24} did not work well on these clays. B_7 would crack and lift up in drying, and peel afterwards, while B_{14} would only crack.

A substitution of Florida kaolin for Graham kaolin was made. It was found that Graham kaolin No. 1 was a better product than Spencer kaolin. It shows the same properties as Spencer kaolin, and has nearly exactly the same shrinkage. In rational analysis they are nearly alike.

	Graham Kaolin No. 1	Spencer Kaolin
Clay substance	95.88	94.12
Feldspar	0.12	1.50
Flint	4.00	4.68

The results of the substitution, B_{51}, was an improvement over B_7. The only defect was that when the glaze was applied, the engobe would blister. After some experiments it was found that a combination of engobe B_{51} and B_{24} would give good results.

Engobe B_{56} which was adopted as satisfactory for both the Colchester and Tennessee clays had the following composition:

Florida kaolin	32.5
Graham kaolin	37.5
Brandywine feldspar	20
Commercial whiting	10

This engobe worked well on dry and green body; it could be dipped in and outside at the same time; and the burning was all done in one fire.

As it seemed strange that flint could not be used so far in engobe, I have started experiments on that subject, but as yet I have been unable to finish the same or come to any conclusion.

As stoneware glazes have a tendency to pin-hole, it often happens that the engobe underneath will be discolored in use, by absorption of dirty liquids through the glaze, and with a view to obviate this trouble, I have started another set of experiments which will give vitrified engobes on stoneware bodies. The results so far have been very satisfactory, and I have produced two engobes which get around this trouble.

One experiment was started on the line of vitrifed bodies, and results were obtained with a formula of the following composition:

$$1\ K_2O \quad 2.15\ Al_2O_3 \quad 10.80\ SiO_2$$

and the other experiment was started on the stoneware glaze line. It had the formula

$$\left.\begin{matrix}0.50\ CaO \\ 0.50\ K_2O\end{matrix}\right\}\ 1.00\ Al_2O_3 \quad 4\ SiO_2$$

All experiments were glazed with the stoneware glaze as mentioned before and burnt between cone 6 and cone 8 heat.

DISCUSSION.

Mr. W. J. J. Bowman: I do not know that I have anything to add to that paper. My experiments in a certain way have followed much the same lines as those outlined in the paper, and his experience has been mine. I would like to say that I think it is necessary to have a proper body underneath, as well as a proper slip. In other words, I think there are certain bodies to which we cannot make a slip stick. At least, that has been my experience.

I would like to raise one point, and that is whether the author means that the slip comes off in firing or whether it comes off when applying it, or drying? He mentions in one place that the slip peels off the outside. In some cases, I find it will do this, if the shrinkage of the slip is not sufficient in the green state; in other cases, it will stay on in the green state but come off when fired, which is due to quite another condition. One fact which I have experienced and never could understand is this: I have had a case where the shrinkage of two different bodies would be entirely different and yet the same slip would apparently work alike on either. The shrinkage of the body did not seem to take much effect on the slip. It would shrink down with one body, which shrank twice as much as the other.

Professor Orton: Do you mean the shrinkage occurring in firing or in drying?

Mr. Bowman: In firing. One would shrink an inch to the foot and the other an inch and a half, and yet the same slip would apparently work as well on one as the other.

Mr. Stanley G. Burt: The slip, however, would have a still greater contraction than the body in both cases.

Mr. Bowman: Yes.

Mr. E. C. Stover: Is not the explanation to that, that the deficiency, as it were, of certain ingredients in the mixture is supplied by an excess of it in the body? I think the line of experiments made by Mr. Binns seemed to show that. Under a very hard fire, the mixture of engobe being deficient in one of the ingredients, and there being an excess of that in the body, the hard fire will make it borrow, as it were, from the body and therefore change its shrinkage and make it fit the body.

Mr. Burt: I think the slip undoubtedly undergoes a chemical change, much as the glaze does. When you get a slip which has an excess of contraction, the moment you have vitrification of the slip, there is an incipient action on the body; so if the the slip vitrifies, it has entered into a certain chemical combination with the body. As long as you have a porous engobe, there is a mere mechanical bond which is easily broken; but as you approach vitrification of your slip, you get a chemical bond which is more difficult to break.

Mr. Bowman: The question I raised was how the slip adjusted itself to the different shrinkages and did not tear.

Mr. Burt: It has a certain elasticity, you know. If you should go beyond that limit of elasticity of the slip, it would tear.

Professor Binns: I think the probable explanation is that the slip in the first place was adjusted to the larger body. If it had been adjusted to the smaller shrinkage, it would scarcely have accommodated itself to the larger. The extra shrinkage could be taken up by the body, but if it had been adapted to a smaller one in the first place, it would peel on the larger. I will ask Mr. Bowman if he did not find, after all, that the cohesion of the slip and the body was less important than the cohesion of the glaze and the body—providing the upper and lower agreed, the intermediate layer did not so much matter? If the glaze agrees with the body, the slip can have a wider range. And if you have a glaze

which will shiver on the main body, even if you apply a good slip to it, it will shiver, the slip peeling right off with it.

Mr. Ray T. Stull: I happened accidentally to produce something in a recent experiment which may throw light on the subject. I made a China body and casted two small vases from it, using the same mould. One I packed in flint, inside and out, and burned to cone eight. The other I did not pack in flint, and burned to cone four. To my great surprise, the one burned to cone eight was larger than the one burned to cone four. The flint on the inside of the one finished at cone eight prevented shrinkage, while the one not packed in flint was perfectly free to shrink.

Professor Orton: I am glad Mr. Stull rose to make that point, as I was about to ask him to do so. It seems to me a very pretty little point with a clear application in this connection. He showed me these trials, and I was surprised to find such difference of size in two vases, cast in the same mould, of the same slip. Both were vitrified, both thoroughly hard, both translucent. The quality of the translucency in the one which shrunk the least, was somewhat deficient as compared with the other; but this may have been due to the fact that the flint packed inside of it had adhered slightly to the inner surface, and did not come off perfectly clean, and thus made a slight lack of translucency. You can see the shadow of your finger through the walls of the vase in both cases. The one which had a mechanical bar to shrinkage, underwent vitrification without condensation, and the one which had no bar to shrinkage, underwent vitrification and condensation as well. If this would occur in the case of Mr. Stull's porcelain body, I do not see why it would not work in the case of a stoneware slip.

Mr. D. C. Thomas: Which of the two vases shrunk the most ?

Mr. Stull: The one finished at cone four, which had no resistance to shrinkage. I might add to that, that the difference in shrinkage between the two was at least three per cent. The one burned to cone four shrunk at least three per cent. more than the one burned to cone eight—I presume more.

Mr. Burt: I would like to ask Professor Orton how that bears on the question of Mr. Bowman's.

Professor Orton: We have here a case where a porcelain vase, not dissimilar in composition to the stoneware slip, had a mechanical bar to shrinkage in the shape of a whole lot of flint packed inside of it. That vase was carried up to the vitrification temperature, where ordinarily it would shrink greatly. The little vase which was not packed with flint had an opportunity to shrink and did not shrink. The other one which could not shrink, went on and vitrified all right, and became translucent, showing that the chemical reactions took place in their usual order, but that the vitrified walls of the vase were sufficiently viscous to take the size of the mass of flint which they held inside.

Now, Mr. Bowman says that he put a slip, which we will assume is similar to a porcelain body—(I don't know how closely similar, but of the same general class of substance)—he put this slip on two bodies. One body shrinks a good deal and the other less. It stays on both without rupture, and he asks why. It seems to me that the cases are parallel. There is a mechanical bar to shrinkage in both cases. In one case it was the flint with which the little vase was filled, and in the other case it is the comparatively non-shrinking body upon which the slip is fastened.

Mr. Burt: That would necessarily assume that the high shrinkage did not take place until after vitrification had taken place; because, as I said before, there is nothing but a mechanical bond between the clay and the slip and before vitrification any tension of any extent would very easily rupture that bond. So if we are to assume that vitrification of the slip enabling it to hold to this body is due to the fact of a bond in any way equivalent to that between the vase and the solid body of flint, we must necessarily assume a very firm bond to the clay underneath, and I do not think we can consider that possible until vitrification has set in. I understand that the bulk of shrinkage takes place at the vitrification point, not before. So you have got to get your bond on the clay before vitrification; otherwise you will have a tear.

Professor Orton: I do not see it that way. It seems to me the shrinkage undoubtedly takes place *during* the process of vitrification, and there is no need of a bond previous to that point, except as the drying shrinkage may require it, and from the minute you begin firing until vitrification sets in, there is little change in volume. If the slip and stoneware were separate, they would probably shrink differently, but being fastened on each other, the bond develops by vitrification between the two fast enough to neutralize the strain produced by this disparity in shrinkage rate which would occur if they were free. That is the way I explain it — it is a sort of progressive attack on the body, as well as porgressive change in the slip itself.

Mr. Burt: I feel that Prof. Orton's explanation is undoubtedly correct. If we make a cone from a glaze mix and completely fuse it, we have as a result a small drop of glaze. Take the same amount of glaze, spread it on a piece of ware, fire it, and we have this glaze extended over a large space. This seems to me much the same phenomena that we have in the vitrified slip. We may apply our glaze to a burnt piece or to one of raw clay. It will hold equally well. The same is true of an engobe. It may be applied on the clay or on the biscuit, and will hold equally well in either case, provided it is properly compounded. It is absolutely necessary, however, that there be a proper mechanical bond between body and slip, or body and glaze, when vitrification sets in. If this does not exist, a glaze will crawl and an engobe peel.

Seger says, that in adjusting an engobe to a body, the rational analysis of each should be carefully considered. I had hoped Mr. Geijsbeek would give us his experience on this subject.

Professor Binns: I think there is an interesting connection between that experiment and the general practice in porcelain factories where flint is commonly used to hold the shape and prevent shrinkage of the ware. And it would be an interesting study, in this example, to make a microscopic examination of the structure of the two vases. It seems to me possible that the larger body is less dense than the

smaller. But it shows the flexibility in vitreous bodies which can be influenced by mechanical means. I think if the fire had been carried a little further the larger vase would have burst. I also think it probable that cone eight heat was not thoroughly reached in the center of the bed flint. You know that heat penetrates slowly into the center of such a mass. You could not know whether that temperature was in the center of the flint unless a pyrometer could be inserted.

Mr. Stull: I think the comparison I made was a fair one. Both vases were fired in a small sagger and in the same kiln and under similar conditions. Even if the cones did not correctly represent the temperatures, the latter were at least comparable.

Mr. Everett F. Townsend: I would say that there is about the difference of one cone between putting tile in a sagger with sand, and putting it in without sand. The shrinkage in the two cases differs by about the same amount as that occasioned by one cone's change in temperature.

STONEWARE GLAZES.

BY

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The term stoneware, as popularly used in this country, means a grade of pottery made from a natural clay or mixture of clays, which is burnt to vitrification, and in which the body and glaze are matured in one and the same fire. The articles usually made are crude jugs, crocks, receptacles for fruits and dairy products; latterly the line has been increased to include many kitchen, table and toilet articles; in addition, a small quantity of stoneware is employed by the chemical industries, for receptacles, stills, worms, stopcocks, conduits, strainers, and similar wares. This constitutes no item in the general trade, but brings prices far above those of general stoneware. In this country, however, the stoneware has not yet attained the reputation for excellence that is enjoyed by the Flemish, German or Japanese products.

The nature of the body used in the manufacture of foreign stoneware is unknown to the writer, but two receipts given as German stoneware bodies, were found to read as follows:

I.	English Blue Clay	25%	II.	Plastic Fireproof Clay	36%
	Kaolin	32		Kaolin	30
	Flint	35		Flint	30
	Spar	8		Spar	4

The American stoneware, with perhaps one or two exceptions, is made from either a single low-grade or No. 2 fire clay, or a mixture of two or more such clays. In nearly all cases the body may be said to be a native, yellow burning fire clay, containing enough fluxes to vitrify it to a blue-gray dense body at about cone 8. It is exceptional, if at all, that a prepared body like those cited is used.

I have also seen German stoneware glazes that figure out to be high-fire fritted glazes. An illustration of these glazes is as follows:

Fritt —	Red Lead	9.1%
	Quartz	32.7
	Borax	27.3
	Whiting	18.2
	Kaolin	12.7
		100.0
Glaze —	Fritt	80%
	Spar	12
	White Lead	8
		100

On calculation, the formulæ of fritt and glaze are found to be respectively:

$$\text{Fritt} - \left.\begin{matrix} 0.186\ PbO \\ 0.244\ Na_2O \\ 0.620\ CaO \end{matrix}\right\} 0.168\ Al_2O_3 \left\{\begin{matrix} 2.20\ SiO_2 \\ 0.49\ B_2O_3 \end{matrix}\right.$$

$$\text{Glaze} - \left.\begin{matrix} 0.20\ PbO \\ 0.21\ Na_2O \\ 0.53\ CaO \\ 0.06\ K_2O \end{matrix}\right\} 0.20\ Al_2O_3 \left\{\begin{matrix} 2.23\ SiO_2 \\ 0.42\ B_2O_3 \end{matrix}\right.$$

The nature of the body and glaze, as given, suggests a ware of much higher grade than any American stoneware. I know of only one instance where an American stoneware potter uses a lead glaze, and I do not know of any that use a fritted glaze.

It must be that in Europe, "Stoneware" includes higher grades of ware than is generally accepted as such in this country.

The nature of the body, as well as the single burn with its slow heating and cooling, precludes the use of lead and boracic glazes from a chemical standpoint. The low price obtained for the ware and the consequent economy of manufacturing, necessitates a glaze that will mature with the body, giving a smooth, glossy coating without having flowed, and which will not be contaminated by direct contact with the fire gases.

Such glazes as are used by American stoneware potters may be classified as follows:

I. Salt Glazes
II. Slip Glazes
III. Bristol Glazes { Clear / White / Colored

SALT GLAZES.

Salt glazing, while fast disappearing from use in the stoneware industry today, is worthy of consideration because of its very interesting chemical reactions and its past importance.

To be salt glazed, the ware is set more open than the ware which is coated with a slip or bristol glaze, so as to expose all the surfaces to be glazed to the free action of the fire gases. The inside of the pieces are glazed with a slip glaze, and are not exposed to the action of the salt vapors.

The heat attained is that sufficient to almost vitrify the body. This is essential in successful salt glazing, for the alkali fumes would penetrate a soft porous body and not give the desired fluxed or glazed surface on the ware.

When the trial pieces drawn show that the body has attained the required density, and the fires have burned clear, salt is thrown onto the fires, either a trifle damp or with wood. Fumes of Na Cl and water vapor are thus generated, the water vapor decomposing the fumes of sodium chloride, setting the alkali free in the kiln to attack the exposed surface of the ware and fusing it into a thin coat of glaze.

The glaze thus formed must naturally approach the formula of slip and Bristol glazes, deriving the necessary Al_2O_3 and SiO_2 from the body. For successful salt glazing the clay must have approximately the same proportion of Al_2O_3 to SiO_2 as is found to exist in the slip and Bristol glazes, viz: from 1:5.5 to 1:7.

SLIP GLAZES.

Slip glazing is the application to the surface of the ware of such clay or clays as will fuse and form a glaze at the heat sufficient to vitrify the body. Such fusible clays are of the ferruginous type, burning to either a red, brown or black.

The Albany slip is widely known for its uniformity in point of fusion and in color. It is a clay mined near Albany, N. Y. Its chemical formula when burned, as given by Langenbeck, is

$$\left.\begin{matrix}0.1954\ K_2O\\0.4592\ CaO\\0.3454\ MgO\end{matrix}\right\}\left.\begin{matrix}0.6080\ Al_2O_3\\0.0810\ Fe_2O_3\end{matrix}\right\}8.965\ SiO_2$$

Slip clays possess some subtle quality given by nature that render them impossible of duplication by mechanical mixtures of the same formulæ. Starting with a ferruginous clay as a basis, adding such ingredients as to duplicate the formula of the natural slips exactly, one would not obtain a glass like or equal to that of the slip glaze.

Albany slip produces a glaze that stands well upon almost every clay body, prepared or natural, without crazing or dunting. It has been observed that lead glazes containing 50 per cent. or more of Albany slip will stand a greater variation in the body than will a lead glaze without the slip clay. What that subtle factor is which renders this clay so valuable and which refuses duplication by mechanical means, is not known.

BRISTOL GLAZES.

A Bristol glaze is one composed essentially of silicate of the alkalies, alkaline-earths and alumina, applicable to green ware, and capable of being burned in open fire with slip glazes. Bristol glazes may be either clear and colorless, white, or colored. The study of clear and colored Bristol glazes will not be taken up at this time.

WHITE BRISTOL GLAZES.

The white Bristol glaze from a scientific standpoint, is a new field for research. Its application is so recent that the ceramic chemist has had hardly time to analyze the scientific causes for its various behaviors, or to ascertain the workable limits of its chemical formula for practical work.

I have not been able to myself carry out any extended research on this subject, but I have at hand the record of a large number of experiments made by a former assistant, with whose methods of work I am familiar, and in addition, I have the trial pieces numbered to correspond to the record. While I would have planned my series differently in some respects, if attacking the problem at the beginning, I still feel that a good deal of value can be gotten from the study of the matter already at hand.

The following notes are therefore to be considered as a digest of these experiments, supplemented by such opinions as my own experience has confirmed in the premises:

I. Oxygen Ratio in White Bristol Glaze.

In the hope of eliciting facts by comparing the oxygen ratio and the ratio of Al_2O_3 to SiO_2 in the glazes of which I have samples at hand, I calculated out the ratios and tabulated them. While it was not found that any definite oxygen ratio, and silica alumina ratio could be said to be the best, it was evident that the best Bristol glazes had an oxygen ratio, ranging between 1:2 and 1:2.5, and silica alumina ratios ranging between 1:5 and 1:7.

From Table I, given later, it is to be noted that even with the oxygen ratio with the limits that seem to be best, the glazes are not good because of physical peculiarities. This illustrates the fact that a Bristol glaze stands between the lead glazes, that are under definite chemical control, and ceramic bodies and pastes that obey chemical laws in but a small part. Chemically, we can increase or decrease the fusibility of a clay body, but the physical properties such as shrinkage, etc., are beyond chemical calculation. It can be said that this or that oxygen ratio is the best in lead glazes, but such is not the case in Bristol glazes.

However, the trials in table I, whose oxygen ration are within the limits of 2—2.5 are better matured and have a better gloss than those of higher or lower oxygen ratio.

II. Al_2O_3 in White Bristol Glazes.

Al_2O_3 in Bristol glaze performs the same function as in lead glazes, being an intermediary element between the fluxes and the acid. In addition to this chemical function, it has a physical function which enters but little into that of a lead glaze, viz: regulation of shrinkage and maintainance of a viscous paste.

It was believed that crawling and pinholing was more prevalent where the Al_2O_3 was high. Desiring to obtain the evidence indirectly, a number of trial pieces showing these defects were chosen and their corresponding formulæ, oxygen ratio, and silica-alumina ratio tabulated.

TABLE I.

No. of the Glaze	Equivalents of the Ingredients.					Oxygen Ratio	Silica-Alumina Ratio
	K_2O	ZnO	CaO	Al_2O_3	SiO_2		
21	0.444	0.444	0.111	0.555	4.44	3.333	8.00
22	0.50	0.388	0.111	0.6	4.44	3.13	7.26
23	0.555	0 333	0.111	0.666	4.44	2.90	6.66
113	0.50	0.40	0.10	0.55	4.00	3.01	7.27
114	0.55	0.40	0.05	0.6	4.50	3.21	7.50
177	0.50	0.40	0.10	0.6	3.21	3.28	5.33
178	0.50	0.40	0.10	0.65	3.30	2.23	5.07
179	0.50	0.40	0.10	0.70	3.40	2.20	4.85
180	0.50	0.40	0.10	0.75	3.50	2.14	4.66
199	0.50	0.40	0.10	0.55	4.00	3.01	7.27
231	0.50	0.40	0.10	0.55	3.25	2.45	5.90
347	0.50	0.40	0.10	0.70	3.60	1.16	5.14
424	0.40	0.45	0.15	0.55	2.93	2.22	5.36
443	0.50	0.40	0.10	0.60	3.61	2 57	6.00
444	0.50	0.40	0.10	0.60	3.80	2.70	6 33
445	0.50	0.40	0.10	0.60	4.00	2.85	6.66
446	0.50	0.40	0.10	0.65	3.60	2.44	5.33
447	0.50	0.40	0.10	0.65	3.80	2.57	5.84
448	0.50	0.40	0.10	0.65	4.00	2.71	6.15

As it is impossible in commercial work to glaze all the ware when at "leather hard" condition, it has become the general practice now to dip the ware when "bone dry," although the finished ware is better if dipped when "leather hard." These trials were dipped when "bone dry."

These trials bore evidence to correctness of the assumption that crawling, in most cases, is due to too much clay. Too much clay means too much shrinkage in clay state, either on drying floor, or in kiln, or both. Too much shrinkage causes cracks in the glaze, perhaps not perceptible at casual glance of the operator, but which develop during the early stages of the burning, leaving on the burned ware a bare spot or seam. As the body exposed by these bare spots and seams does not show evidence of having been affected by any fluxing action, it is safe to presume that the glaze had crawled before it had been fused even to the slightest extent.

.55 equivalent Al_2O_3 seems to be the maximum that is safe to use under the existing conditions.

To see if the converse was true, a few trials, free from crawling and pinholding were selected, and their corresponding formulæ found to be as follows:

TABLE II.

No. of Glaze	Equivalents of the Ingredients.					Oxygen Ratio	Silica-Alumina Ratio
	K_2O	ZnO	CaO	Al_2O_3	SiO_2		
54	0.233	0.4	0.366	0.33	1.94	1.3	6.00
106	0.45	0.3	0 25	0.50	3.50	2.8	7.00
107	0.50	0.3	0.20	0.55	4.00	3.01	7.27
110	0.50	0.35	0.15	0.55	4.00	3.01	7.27
111	0.40	0.40	0.20	0.45	3 00	2.55	6.66
116	0.40	0.40	0.20	0.50	4.00	3.20	8.00
570	0.45	0.40	0.15	0.50	3.00	2.40	6.00
575	0.45	0.40	0.15	0.525	3 15	2.44	6.00

There were other trials free from crawling and pinholing, but as they had the same Al_2O_3 and SiO_2 equivalents as those here represented, they were not figured out.

It was observed, also, that when the Al_2O_3 exceeds .55 equivalents, the glaze loses the *clear white* color and takes on a *grayish white* tint.

A Bristol glaze without any equivalents of clay, strange to say, has not the fusibility of those in which .05 equivalents of clay is used, as will be seen by the following:

TABLE III.

No.	K_2O	ZnO	CaO	Al_2O_3	SiO_2	
167	0.5	0.4	0.1	0.5	3.00	
168	0.5	0.4	0.1	0.5	3.25	All Al_2O_3
169	0.5	0.4	0.1	0.5	3.50	derived
170	0.5	0.4	0.1	0.5	3.75	from Spar
171	0.5	0.4	0.1	0.5	4.00	

TABLE IV.

No.	K_2O	ZnO	CaO	Al_2O_3	SiO_2	
172	0.5	0.4	0.1	0.55	3.00	.05 Al_2O_3 derived from Clay, balance from Spar
173	0.5	0.4	0.1	0.55	3.25	
174	0.5	0.4	0.1	0.55	3.50	
175	0.5	0.4	0.1	0.55	3.75	
176	0.5	0.4	0.1	0.55	4.00	

This being a surprise, the samples are submitted for inspection. Those containing the clay are very distinctly better than those without it.

In the Bristol glaze used the most extensively in the Muskingum Valley district of Ohio, the equivalent of clay averages about .14, the glaze maturing at cone 8 to 9.

III. K_2O in White Bristol Glazes.

Potash is generally the only alkali flux used in Bristol glazes. Soda is sometimes introduced by the use of soda spar or ground glass, but these materials are seldom used. The soda introduced by the small amount of borax used in some cases, is too small to exert any important influence.

In feldspar, the stoneware potter finds the potash in the best and cheapest form for his use.

The practical limitations of the use of feldspar is governed on the one hand by the amount of Al_2O_3 permissible in the formula, and by the solvent action of the potash upon ZnO ; and on the other hand by the potash required to flux the glaze to a bright gloss at the heat used.

Lead, as a flux in a white Bristol glaze, is not permissible, not only because of its poisonous character, but because of its great solvent action upon ZnO.

The empirical experimenter in increasing or decreasing the fusibility of his Bristol glaze generally attempts to merely raise or lower the percentage amount of feldspar. In some cases, such a course of precedure would bring the desired results. But through chemistry we learn the limits of such variations.

From the formulæ having .55 equivalents of Al_2O_3, which was found to be the maximum that could be worked with safety, the following were selected as being the best:

TABLE V.

No.	Equivalents of the Ingredients.					Oxygen Ratio	Silica Alumina Ratio
	K_2O	ZnO	CaO	Al_2O_3	SiO_2		
172	0.5	0.4	0.1	0.55	3.10	2.34	5.63
173	0.5	0.4	0.1	0.55	3.25	2.45	5.90
174	0.5	0.4	0.1	0.55	3.50	2.64	6.36
175	0.5	0.4	0.1	0.55	4.00	3.07	7.27
176	0.5	0.4	0.1	0.55	4.00	3.07	7.27
199	0.5	0.4	0.1	0.55	4.00	3.07	7.27
281	0.5	0.4	0.1	0.55	3.25	2.45	5.90

As each of the trials in this table was weighed, ground and applied independent of any other, all possibility of an error influencing each one alike is eliminated. Each stands for itself.

It is noted that .5 K_2O is found necessary to bring these glazes to a good maturity at cone 5 to 6, the heat at which all of the trials were burned.

To harden the glaze, lower the K_2O and increase the CaO, but do not alter the Al_2O_3.

IV. ZnO in White Bristol Glazes.

Zinc has a double mission in a Bristol glaze, acting both as a flux and an opacifier. Other than SnO_2, it is the best opacifying element known to the potter. It is a good flux in lead glazes, adding brilliancy to colors. It is also a good flux in Bristol glazes, making a more fusible RO combination with K_2O and CaO, than would K_2O and CaO alone.

It is astonishing to many that ZnO in some Bristol glaze formulæ does not act as an opacifier, but is completely dissolved, leaving the glazes perfectly clear.

Two glazes showing this are here quoted:

TABLE VI.

No.	K_2O	ZnO	CaO	Al_2O_3	SiO_2	
425	.45	.40	.15	.60	3.25	White
466	.55	.35	.10	.45	2.80	Clear

All the glazes that were either clear or but slightly opaque, were selected and tabulated as follows:

TABLE VII.

No.	K_2O	ZnO	CaO	Al_2O_3	SiO_2	Oxygen Ratio	Silica Alumina Ratio
105	0.40	0.3	0.3	0.45	3.00	2.55	6.66
106	0.45	0.3	0.25	0.50	3.50	2.80	7.00
107	0.50	0.3	0.20	0.55	4.00	3.01	7.27
108	0.40	0.35	0.25	0.45	3.00	2.55	6.66
109	0.45	0.35	0.20	0.50	3.50	2.80	7.00
110	0.50	0.35	0.15	0.55	4.00	3.01	7.27
115	0.40	0.35	0.25	0.50	4.00	3.20	8.00
458	0.30	0.35	0.35	0.40	2.50	2.27	6.25
459	0.325	0.35	0.325	0.425	2.65	2.30	6.33
461	0.375	0.35	0.275	0.475	2.95	2.43	6.20
460	0.350	0.35	0.300	0.45	2.80	2.38	6.22
462	0.400	0.35	0.25	0.500	3.10	2.48	6.20

It is noted that, irrespective of the equivalent amount of the other elements, the resultant glaze is either clear, or nearly so, when the ZnO is .35 equivalent or less.

To note the effect of the use of more than .4 ZnO, the following were compared:

TABLE VIII.

No.	K_2O	ZnO	CaO	Al_2O_3	SiO_2	Oxygen Ratio	Silica Alumina Ratio
117	0.40	0.45	0.15	0.50	4.00	3.20	8.00
118	0.40	0.50	0.10	0.50	4.00	3.20	8.00
424	0.40	0.45	0.15	0.55	2.95	2.22	5.36

Of these, 117 is very good except that it is not perfectly matured. It showed no evidence of crawling. The other two crawled.

It is safe to assume, then, that when the equivalent of Al_2O_3 is 0.5, 0.45 equivalent of ZnO can be safely used, but with 0.55 equivalent of Al_2O_3 not more than 0.4 equivalent of ZnO can be used.

To zinc, many ascribe all the responsibility for the manifestations of crawling, but we have not found that to be the case. ZnO and Al_2O_3 alike are responsible and their

workable limits have here been found to be between 0.50 to 0.55 equivalent Al_2O_3 and 0.45 to 0.40 equivalent of ZnO. When the Al_2O_3 is high, the ZnO should be low, and vice versa.

V. CaO in White Bristol Glazes.

CaO is added as the third RO element. It acts in most cases as a flux, but in some it acts as an opacifier also, or at least adds to the opacity of the glaze.

CaO is an alkaline earth, practically infusible by itself, but in a silicate mixture it is readily soluble with a lead flux, and at higher heats, with the alkali fluxes.

It is used in the compounding of a Bristol glaze in the form of ordinary whiting. In some cases, it is derived also from CaF_2, and $Ca_2(PO_4)_2$, but these cases are rare.

The writer has made no experiments with $Ca_2(PO_4)_2$ as a source of CaO but knows of instances where it is used. It is said to add opacity to the glaze.

Fluorspar is an ingredient that needs to be added very cautiously. Its specific weight makes it a troublesome factor in keeping the glaze mixture homogenous in the dipping tubs.

The following trials will show to some extent its chemical action :

TABLE IX.

No.	K_2O	ZnO	CaO	Al_2O_3	SiO_2
562	0.55	0.35	0.10 CaF_2 + 0.00 $CaCO_3$	0.65	3.75
563	0.55	0.30	0.15 CaF_2 + 0.00 $CaCO_3$	0.65	3.75
564	0.55	0.35	0.05 CaF_2 + 0.05 $CaCO_3$	0.66	3.75
565	0.55	0.30	0.05 CaF_2 + 0.10 $CaCO_3$	0.65	3.75
566	0.55	0.30	0.10 CaF_2 + 0.05 $CaCO_3$	0.65	3.75
567	0.45	0.40	0.05 CaF_2 + 0.10 $CaCO_3$	0.60	3.25
568	0.45	0.40	0.10 CaF_2 + 0.05 $CaCO_3$	0.60	3.25

As to color, these trials are whiter and glossier than were those of similar composition, but in which all the CaO was obtained from $CaCO_3$.

Samples of these glazes were so placed as to receive a soft and a hard burn. In the softer burned samples, the pinholes, while distinct, are not so numerous as those on the harder burned samples. This same action of CaF_2 was noticed in vitreous tile bodies.

These trials would indicate that 0.05 equivalent of CaF_2 is the maximum that can be used in these glazes. and that for higher heats (cone 7 and up) its use in vitreous or fusible ceramic mixtures is not permissible.

It is to be noted also, that even with the high equivalent of Al_2O_3 these trials show no tendency to crawling.

The glazes given above contained 0.01 equivalent of of borax. The following glaze was made up without borax as follows:

.25	Eqv.	Feldspar.
.20	"	Common Glass.
.05	"	Whiting.
.25	"	Fluorspar.
.15	"	Magnesia.
.10	"	Zinc Oxide.
.10	"	Clay.
.30	"	Flint.

The formula thus produced is:

$$\left.\begin{array}{l} 0.25\ K_2O \\ 0.10\ Na_2O \\ 0.40\ CaO \\ 0.15\ MgO \\ 0.10\ ZnO \end{array}\right\} \quad 0.35\ Al_2O_3\ \ 2.5\ SiO_2$$

This glaze was burned in the open kiln at cone 8. It did not pinhole in the least, and even with the low ZnO and Al_2O_3, was a fair white Bristol glaze.

VI. SiO_2 in White Bristol Glaze.

SiO_2, as an acid, has the same functions in a Bristol glaze as in a lead glaze. However, as Bristol glazes are not designed to flow, as are the lead and boracic glazes, but rather to be only thoroughly fused to a viscous paste, the acid does not have quite the same influence. Indeed, it will be seen that the best Bristol glazes have a lower proportion of Al_2O_3 to SiO_2 than do the ordinary lead glazes.

While SiO_2 is almost infusible by itself, it is often the case that a glaze is too refractory because of its low acidity, and to soften it, more SiO_2 must be added. The converse is likewise true.

As will be seen by the following formulæ of Bristol glazes known to be in use in factories in different parts of the country, consequently on different clays, the best ratio for alumina to silica is between 1:5 and 1:7:

TABLE X.

FORMULÆ			Salts added to prevent crawling	Cone to which Glaze is Fired	Silica Alumina Ratio
0.407 K_2O 0.194 CaO 0.399 ZnO	0.507 Al_2O_3	2.857 SiO_2		5–6	5.63
0.377 K_2O 0.180 CaO 0.444 ZnO	0.516 Al_2O_3	2.84 SiO_2	0.015 Borax	8	5.5
0.450 K_2O 0.150 CaO 0.400 ZnO	0.6 Al_2O_3	3.25 SiO_2	0.01 Borax	5–6	5.4
0.327 K_2O 0.300 CaO 0.370 ZnO	0.366 Al_2O_3	2.54 SiO_2	3% Borax	6	6.9
0.300 K_2O 0.200 CaO 0.500 ZnO	0.5 Al_2O_3	4.00 SiO_2	.05 Boracic Acid	10-12	8
0.224 K_2O 0.388 CaO 0.386 ZnO	0.3567 Al_2O_3	3.00 SiO_2		7	8.4

Having found that for the conditions under which the trials quoted were made, the K_2O, ZnO and CaO work best when in the proportions respectively of 5:4:1 equivalents, with Al_2O_3 at 0.55 and a silica-alumina ratio of about 1:6, then the following formula may be regarded as the type or standard to be used under like conditions:

$$\left.\begin{matrix}0.5\ K_2O\\0.4\ ZnO\\0.1\ CaO\end{matrix}\right\}\ 0.55\ Al_2O_3\ \left\{3.30\ SiO_2\right.$$

which will develop into a good white glaze at cone 6.

VII. Common Defects in White Bristol Glaze.

I. Crawling.

Crawling may due to any one of the following causes or to the combined influence of any two or more of the conditions, even though they may be present in but a slight degree.

A. Too high in clay, or Al_2O_3
B. Too high in ZnO
C. Too thickly dipped
D. Dusty ware
E. Sweating in drying, or early stages of the burning.

"A" and "B" have been discussed.

"C" is obvious to those who have ever coated green ware with another clay mixture.

"D", while it does not receive much attention from the average stoneware potter, is, nevertheless, the cause of much imperfect ware. Electrical insulating companies who make dry press ware have this trouble to a larger entent than do the stoneware companies.

"E" is better seen by the following citation:

For economy of kiln space, and convenience as well, coffee pot lids were often placed between kettles after having been dipped, and hacked in bungs of from 20 to 25 each. The kettles and lids contained such moisture as was absorbed in dipping. These bungs were left on the open drying floor for three or more days. The bungs were then carried intact to the kiln.

It was observed that on over 80 per cent. of those lids, the glaze would be peeled in places and in some cases slipped off the lid entirely. That same glaze under normal conditions of drying, worked beautifully, having very little crawling.

The crawling in this case was obviously caused by the thorough sweating these lids must have been subjected to, being between the crocks where no ventilation was possible.

It is a fact that in a steam dryer with fair ventilation, tile containing from 6 to 10 per cent. of moisture when

placed within it, will sweat until they appear to be very wet, after having been there 24 hours.

The case just cited, while an extreme case, illustrates the effect which too little draft in the "drying off" stage of burning will have.

Most stoneware potters insist upon "keeping the kiln hot", that is not to allow them to stand longer between the burnings than necessary to draw and set. It is the experience of potters, known to the writer, that if the kiln is cool when set, crawled glaze can be depended upon.

This is obviously another case of sweating. The kilns have drawn moisture, which with the moisture from the ware, causes sufficient steam to thoroughly sweat the ware.

As an antidote for crawling, some soluble compound such as $Na_2B_4O_7$ or Na_2CO_3 and Plaster Paris slop are used. To what the counteracting effect of these compounds may be due, is not positively known. Some contend that in drying the soluble salts crystalize, so interlocking themselves as to bind the whole into a coating that will not be apt to crawl.

II. Pinholes.

Pinholes are more often due to imperfect finishing of the ware. Small cavities left in the surface of the ware, become filled with air when the piece is dipped. The air is expelled, blowing a hole through the glaze that has covered the cavity; the result being a pinhole in the finished product.

III. Dryness.

Dryness may be caused by two conditions.

First. By too rapid raising of the heat in the early part of the burning.

Second. By too low or too high SiO_2 as in the last formula given as being known to be in use.

IV. Dusting

After the ware has dried, it is rubbed by the hands and clothes of the kiln setter and if the glaze is too dry, enough will be rubbed off to cause defective ware.

Dusting is prevented by the use of Ball clay in place of China clay or $Na_2B_4O_7$ in place of plaster slop, or use the plaster paris before it has had time to harden.

DISCUSSION.

Mr. Otto Hensel: Last year there were some experiments presented here regarding the use of magnesia as a flux, but no special attention was called to the value of magnesia as to other properties desired in a glaze, such as prevention of crazing and preservation of color. If anything was said on this point, it was mentioned incidentally, but it was not stated that magnesia is more valuable than lime in making a glaze having a greater range of use. In Germany they are beginning to pay more attention to the value of magnesia than they did before. As regards magnesia in clays, from another aspect, Dr. Mäckler states in a technical periodical, that he has subjected some fireproof terra-cotta lumber to chemical analysis. These particular hollow blocks are imported into northern Germany from southern Europe, and are in great favor because they have very thin walls and are nevertheless quite true in shape. He inclines to the belief that their high content of magnesia, estimated at about 9 per cent. carbonate of magnesia in the raw state, gives properties to the clay, such as lime alone will not furnish. He announces that he is going to make a further report on the effects of magnesia. I have noticed that clays in which magnesia is as high the lime are very valuable in holding up their shape in firing, and are perhaps of better quality when burned than if they had been lime in place of the magnesia present.

Professor Orton: Mr. President, I want to say, in corroboration of what Mr. Hensel has called attention to, viz: that the presence of magnesia may be a source of peculiar value to the potter, that I made a series of tests in the way of regular class work, in which we attempted to make a slip like the Albany, beginning with common red shale as a base, and adding those ingredients which the shale did not have in itself. It was not intended as a practical process, because it is cheaper to buy Albany slip than to make it; but it occurred to me as a good problem in the way of drill in the preparation of ceramic mixtures. So we proceeded to alter the red clay to make it do what Albany slip would do as nearly

as we could. In that series of tests, which were only moderately successful, we used whiting to supply the lime and, incidentally magnesia. We construed the lime and magnesia as equal and used lime in place of magnesia; and we were constantly troubled to get anything like the desired color effects where the iron used was merely equal to that in the Albany Slip. My class in experimental work is repeating the work which we did several years ago, with a different clay. We are taking red Bedford shale which is better suited because of more alkalies and more iron; and we are doctoring this red shale into as close an approximation of Albany slip as we can, but we are using dolomite as a source of lime. While we have no results as yet to bring forth, I believe we are rather in the way of progress there, because I believe that magnesia does have a different effect on the color of iron; or, rather, it fails to mask the effect of iron, as lime alone does.

I want to say that this paper has been a surprise to me. Mr. Purdy has been changing his location from Ohio to Illinois, and the paper has been prepared in the last three weeks, in intervals of moving. He has put together more concrete information on the subject of stoneware glazes than I have seen in any one place before. And, further, the results he exploits, while not prepared, perhaps, in the way of a regular series, beginning with the changing values of one element at a time, are a large number of them glazes in actual use in representative stoneware factories at the present time, and gathered by him from a wide knowledge among stoneware men themselves; and I submit that when the paper is put out in printed form, it will be a compendium for stoneware men which they should be exceedingly glad to get, if they are wise. Furthermore, I believe that the same class of glazes are capable of a wide extension in use by enameled brick men. I believe it is true that there is no cheaper glaze with which to coat an architectural product than those of this general lime; therefore, to enamel brick men and terra-cotta men, I think this paper is of very substantial value.

Mr. Burt: My attention was called some time ago to the fact that a high content of zinc in a glaze appeared to render the glaze less acid proof. Four-tenths equivalents of ZnO is rather a high content. Have any of the members experience enough to say whether that amount does carry with it some danger as to the stability of the glaze, as to being acid proof?

Mr. Arthur S. Watts: I have had a little experience in that line. I used for four-tenths of zinc on a glaze for porcelain, fired at cone twelve, single burn for body and glaze. It was repeatedly tested with acid, and never showed any effect of attack. Of course, the temperature used was high. Whether at a lower temperature like ordinary stoneware heat, it would be affected, I do not know.

NOTE ON FRITTING.

BY

STANLEY G. BURT, CINCINNATI, OHIO.

One of the first things I was told on entering the pottery business, was that fritting reduced the contraction of a glaze, and therefore tended to stop craze. I have seen this statement printed many times since. It sounds very plausible. Crazing is undoubtedly due to an excessive contraction of the glaze. Then, if fritting reduces contraction, as it undoubtedly does, why doesn't it stop crazing? I quote from a recently published article on pottery:

"The preparation of the glaze is quite as important, for it is composed of materials subject to great contraction, namely, quartz, feldspar, paris white, borax and a little lead. These, with the exception of the latter article, are fritted or melted in an oven till they run like molasses, they then harden and are crushed and ground in water. Having lost much of their contractility, they are now ready to coat the ware and will fuse and harden on the biscuit without crazing."

As I said in my paper last year, we have in the pottery business two distinct contractions which I called permanent contraction or shrinkage, and constant contraction. If we fire a piece of clay until it gradually approaches fusion, the pores close and we say the clay shrinks. This is permanent contraction. When once fired it can never go back to its original size. Now, if the fired piece is heated it will expand, and on cooling will contract to its usual size. This I call constant expansion and contraction; it is going on all the time, and for the same degree of heat is a constant quantity.

Much the same thing occurs in fritting a glaze; it loses its water of crystallization, carbonic gas, etc.; it fuses, the

pores close, and we have permanent contraction: that is, shrinkage; It has undoubtedly undergone a great permanent contraction, but what has this to do with the coefficient of its constant contraction? If we melt a glaze mix on the inside of a crucible, and call it a fritt, how is it different from the same mix fired at the same time on the outside of the crucible, and called a glaze?

So if we take a glaze mix and fritt it, this mix fuses and contracts; but this is merely permanent contraction, which, as explained, may have no effect whatever on the final coefficient of the glaze, so that a fritted glaze may craze as well as a raw glaze.

In statements like this, that have been handed down from generation to generation, there is generally some basis of fact, and I believe it is undoubtedly true that fritted glaze is less liable to craze than a so-called raw glaze. But, the reasons for this fact are entirely different from those assigned above.

Leaving aside the fact that boracic acid, one of the best means for eliminating craze, that is reducing the coefficient of the glaze, can only be used in a fritted glaze, and that almost all fritted glazes contain it. Let the same glaze be tested raw and fritted, and I think the fritted sample would undoubtedly show the lower coefficient.

A glaze is a chemical combination. All such combinations take more or less time for their completion. The perfect formation of a glaze takes quite an appreciable length of time. We do not, in firing, rush our kiln up to a maximum heat and then stop. We either approach gradually, or else hold the kiln at its full heat in order to allow time for the glaze to complete its chemical combination, or, as we say, its fusion. An under-fired glaze shows undissolved particles in suspension, but how is it with an over-fired glaze? In this case, having completely combined all the ingredients of the glaze mix, it begins to dissolve those of the body. Of the body ingredient readily taken into solution, flint is the chief, and this flint, taken from the body, reduces the glaze coefficient.

With a fritted glaze the chemical combination has already been completed, so that it fuses much more readily and starts at once, as it were, to take up flint from the body. So in the same interval of time, it will be able to take up much more than the raw glaze, and its coefficient will be brought lower and the danger of craze much reduced.

I feel it is peculiarily the duty of our society to explain pottery dogmas like this, and to make a clear understanding of the facts open to those who care to know them.

OPPORTUNITIES FOR RESEARCH IN A FACTORY.

BY

CHARLES F. BINNS, ALFRED, N. Y.

The common acceptation of the term "research" is, an original investigation which tends to increase the knowledge of mankind, but, while not neglecting this wide definition, the term may be employed as expressing a pursuit of information which shall increase one's own knowledge, even though others may have trodden the same path.

In this sense every experimenter is engaged in research and is adding to his personal store of facts, even though he discovers nothing new. At the same time, even the most modest research must be more than a mere trying of recipes. There are in the ranks of our clay workers, hundreds of men whose love for a recipe passes the love of women, but who never get further than the recipes can take them. The experimenting that is needed is along the line of testing the influence of various minerals and chemicals upon each other under differing conditions, and thus preparing the way for new combinations and perhaps important results.

But the question will be raised, "suppose a superintendent or foreman be successful in some discovery, of what use will it be to him, and will he be ready to impart his knowledge?"

I submit that the question is premature. The spirit of research slumbers in the minds of many. Perhaps it is starved by the exigent demands of other faculties or it may be drugged into quietude by pride or suspicion, for even clay-workers are human and have human failings; but let it be awakened, let an occasional hour be stolen from play and the draught of inquiry be tasted. Then shall the spirit of research awake and arise and difficulties will disappear as dew before the morning sun.

It has often been remarked that the nature of the work to be performed, measures the amount of time available for the performance. No young man, for example, ever pleaded want of time as an excuse for not calling on his lady love, nor will pressure of other work ever keep the devoted experimenter away from his pestle and mortar.

It is quite possible that no material gain — no money gain — may follow new discoveries or new applications of old rules, but he who gains knowledge gains power, and every success, even if unrecognized, places a man just so much further towards his goal. As to the imparting of knowledge, a clay-worker cannot make many trials, without feeling the need of some congenial spirit with whom to discuss the results, and a mutual interchange soon follows.

The common idea of the practical man — why should that expression not be changed? We are all practical men; the term seems, by common consent to belong to the man who has nothing but his practical qualifications to stand upon, no science, in other words — is that investigation is the prerogative of the scientific man, and the college professor. They are not practical, and have nothing to do but weigh and mix. To state that professors have no time for research, would be to controvert my own argument; but this I will say — after wrestling with classes of elementary students and patiently covering the familiar ground of formula and reaction, it is an immense relief to turn to the laboratory table and pursue some inquiry, thought out perhaps, in theory, during wakeful nights or such spare moments as may have been secured.

The experimenter in the factory has one great and far reaching advantage, viz: His kilns are being regularly fired. His trials for the most part are made at the established temperatures of his glost and biscuit ovens, and all he has to do when the trial is prepared is to hand it on to the kilnman.

Those on the other hand, who are popularly supposed to be able to experiment more easily, have to provide themselves with even the simplest appliances—to make their own saggers, to do their own potting, dipping and placing and to fire their own kilns.

The necessary tools for factory research are few and simple. A dozen jars, a pestle and mortar, a sheet of plate glass, a muller, a palette knife, a sponge and a couple of bowls with a good pair of scales make up the outfit. It is best to discard at once the use of pounds and ounces. A set of weighs from 100 grams to .1 gram will permit trials to be made in percentage proportions and the transitions to larger weights is easy. It is perhaps hardly necessary to point out the importance of system in trial making, yet a potter's table, covered with trials of every shape and size, with no distinguishing mark but an arbitrary sign, is familiar.

Clay trials should be made in a uniform mould or die, according to the ceramic field explored; the length should be upward of 10 cm. to allow that measure to be accurately marked as a test of shrinkage. The heat of the oven should be marked, either by anticipation in the clay or afterwards in color. A series number and a specific number should be given, and accurate notes kept. It often happens that a number of trials are thrown away after a few months, simply because the man who made them has forgotten what they were.

When fired, the briquette or tile can be glazed or enameled over one-half of its surface to allow a comparison between glazed and unglazed surfaces.

A corner of the mixing room can generally be set apart for this experimental work and to this spot, as to a haven of refuge after a stormy sea, the harassed superintendent can retire for a moment's breathing space.

It may not be out of place here to suggest some of the problems which may with profit be investigated by the practical potter. In the white ware industry the employment of native materials is an important question. A great deal of English kaolin is being used, and the only reason is that it has always has been so—a reason that might perhaps satisfy an Englishman, but which seems to be out of harmony with American ideas. The rational analysis of the respective clays can always be obtained, and the substitution becomes a matter of accurate adjustment. I am acquainted with potters who pride themselves on using nothing but

American clays, and yet there are others, making similar wares, who claim that these clays are not good enough.

Flint is a material about which little is known. Mr. Burt's paper of last year opened up the subject, and there is room for an exhaustive inquiry as to size of grain, shape of grain, dry or wet grinding and color. In glazes there is scope for unlimited research. Theoretically, many are satisfied as to the cause and cure of crazing, but how many have conducted a systematic series of experiments to support or disprove the theories? The best glaze for under glaze colors is a subject about which everybody wants information, and the best way to get it is to dig it out for one's self. Mr. Mayer gave an important contribution on this point at our first meeting, but the investigation should be pursued at other temperatures than that used by him.

In coarser wares, the problems are at least as numerous and important. Let me instance a few of them: The best structure and composition of a fire-brick to withstand fire and weight; the influence of grog, its proper composition and size of grain; the profitable production of enameled brick; the manufacture of blue paving brick; the successful manufacturer of electric conduits; the resistance of clay ware to electric pressure, and so forth.

No man can afford to stand still; others will not wait for him; but, by thoughtful work, it is possible for everyone to help towards the success of the industry which is his life's vocation.

The facilities of the factory are not to be despised. Even though there be less time for thought and research, less time is necessary for the worker who has a factory at his back can outpace him who has no such help.

If we could persuade each of our factory superintendents to be, even in a small way, an experimenter, and to tell of his work, a vast amount of useful ceramic information would soon be compiled to the great advantage of our industry and the benefit of every clay worker in the land.

THE COMPOSITION OF PORCELAIN FOR ELECTRICAL PURPOSES.

BY

ARTHUR S. WATTS, FINDLAY, OHIO.

Connection with and interest in the porcelain insulator business has impressed me with the fact that while the products of some companies stand very high tests and seem in every way ideal insulators, the products of some other companies are well nigh worthless.

What is the cause? What are the essential constituents of an electrical insulating porcelain?

Those who know, if any know, do not seem disposed to distribute the information. Hence, I have decided to pursue such a line of investigation as will determine, if possible, what these essentials are.

I give you my work along this line. The results as I find them may be disproven by later investigation, but if only a foundation for the later work may be laid at this time, I will consider myself well repaid for the time invested.

General Considerations on Electrical Porcelain Bodies.

I quote as follows, from Herzog and Feldman on the subject of electrical porcelain: "A porcelain mass will be the more resistant, the higher the proportion of kaolin and feldspar; also the finer the grain of the kaolin used, the higher will be the maturing point of the porcelain produced."

The leading materials used in porcelain manufacture are certain kinds of kaolin, which are very slightly plastic, quartz and feldspar. Of course ordinary feldspar displays basic properties in the fire, uniting with silica to form silicates, thus producing an intimate mixture of all silicates. Therefore, the contents of kaolin must be in a definite ratio to the quartz and feldspar in regard to the insulating properties.

Too much quartz makes the body too little plastic, thus making it difficult to work, and after burning, the ware shows impressions and some warping due to the tendency of the clay substance to the contract.

Too high a content of feldspar makes the body soft and liquid, and increases the tendency to crack.

A body low in feldspar does not produce sufficient vitrification and hence decreases its insulating efficiency.

A porcelain body may shrink 15 to 18 per cent., and it is very important that the exact degree of shrinkage be known. No crazing or shivering is allowable in the glaze used on ware for electrical materials. If the glaze is too infusible the ware will be dull, and if too fusible it is likely to be covered with blisters. Investigation has proven the condemnation of glazes containing metallic oxides to be unfair. Metallic oxides, when used in glazes combine with the silica, forming silicates.

Probably in no line of electrical investigation have greater progress and more varied experiments been made than in the shape and size of electrical insulators. The plain bell which is used for low potentials and which is also practical and satisfactory for alternating currents up to 1000 volts, does not meet the requirements of modern high voltage transmissions.

The first step in the way of improvement was made in 1857 by Borggreve. The product was satisfactory so far as electrical and mechanical requirements were concerned, but failed owing to the fact that the interior space was square, which gave rise to uneven tensions in the body and frequent fractures.

The second step in the way of improvement was the production of a very long insulator having a very narrow cross-section. It was the idea of the inventor to increase the resistance by means of a long arcing distance. The insulator produced was of clear glass and suffered from the contraction and expansion of the iron bolt used to connect it to the cross-arm and which became intensely hot from the sun's rays on the insulator. This condition caused the introduction in 1895 of the double bell insulator of porcelain.

In this, the interior cylindrical part of the insulator is covered by a second bell which prevents the radiation of heat and also the formation of dew.

Up to a tension of 3000 volts, the ordinary double bell 75 mm. wide and 100 mm. high suffices, but above this voltage the question of further improvement arises.

In 1869 Lenoir and Prudhomme obtained a patent for the use of oil in insulator bells. Insulators of this type were used in constructing the famous line between Lauffen and Frankfurt. They proved impractical, as the oil must be replaced frequently and in case of rain it was often replaced by water.

In constructing the Kaiser Wilhelm Canal Line a triple bell insulator without oil was used and proved satisfactory even when exposed to salt spray. This type is now commonly in use for high voltage transmissions.

At first the insulators were glazed and burned resting on the bottom edge of the bell, but the roughness of this edge seemed to cause the collection of drops of water and to prevent this, the insulator is now glazed all over, the glaze removed from the head and the insulator burned inverted. Very recently it has been discovered that by glazing the inside of the insulator in the threaded hole, the insulator is made much more resistant and need not be so thick.

The following table of the Delta Bell Insulator I take from Herzog and Feldman "Electric Lighting Installation:"

Weight of Insulator	Potential Voltage to be Carried	Height	Diameter	Shortest Distance Allowable		
				From Insulator to Nearest Object	Between two Parallel Lines	From Insulator to the Ground
grams		mm	mm	mm	mm	mm
1200	6000	115	145	200	400	190
2500	12000	150	190	260	520	220
4800	25000	170	250	360	720	280
7000	50000	225	250	460	920	370

The weight of the porcelain used in the insulator is not of the greatest importance, but rather, the quality of the porcelain and the kind of glaze.

Manufacturing Processes.

Just here a word regarding the working of porcelain bodies may not be out of place. In Europe, insulators are made by all five of the clay working processes, viz: turning, jollying, pressing, casting and dry pressing. I am familiar with all of these processes except casting, but am not prepared to pass judgment on the superiority of any given process of working, as of course the product and its shape and size would greatly influence the method chosen. Dr. Seger, in his work, shows very conclusively that a semi-dry process, in which the material to be pressed contains the highest per cent. of water allowing of good pressing, gives the densest and strongest body.

Turning of large insulators in done similarly to all clay turning, the product being made mouth up and the top finished when the ware is sufficiently dry to permit of its being inverted. In this class of ware, much trouble is experienced in making the screw-thread for the pin which is to support the insulator. This thread is made by means of a threaded steel tube which is provided with a handle on one end and which is slowly turned in and out. In place of a full steel tube a segment may be used. Automatic boring apparatus is now being applied for this work.

Jollying is now largely replacing turning although it does not insure as dense and uniform a body as the former process.

Pressing is done in molds. The clay is placed in the mold in a lump and by means of a plunger is forced to assume the shape of the inside of the mold for its exterior while the plunger shapes its interior.

Dry pressing produces more exact shapes than any other method. The molds used are of steel and are very expensive. The body must be in such shape that it will not stick to the mold. To obtain it in this condition some factories

use the following process: The clay after drying and being thoroughly pulverized, is placed in wet cement or plaster boxes and allowed to absorb 10 to 20 per cent of moisture. The mass is then sieved thoroughly. Ten kilograms of the body will contain about 1 to 2 litres of water. Of course this proportion must be maintained exactly in order to insure a uniform shrinkage. The molds are sometimes complicated and are often composed of insertions. The piece of ware produced is threaded by means of a screw inserted in the bottom before the clay is introduced and which is screwed out rapidly by means of a crank. The tendency is to make insulators without threads, as this has always been a great source of trouble and loss in manufacture.

Selection of a Standard Porcelain as a Basis for the Experimental Work.

In outlining the work that follows, it seemed necessary to choose some type or standard of porcelain which I could use as a basis from which to map out varying series. The wisest course here seemed to be to procure a large number of standard porcelains, and to obtain from these by comparison the most characteristic type of porcelain now in use.

In compiling the following list of porcelains, I have not stated the present use of the body, but will state that the list includes only practical workable porcelains that have been, or are now, in actual use.

I wish to further state, that in my opinion such a list is valuable as a comparison and not as a basis from which to compound practical porcelains. Acting on Professor Binn's suggestion,* I shall take Al_2O_3 as unity in these formulæ.

* The Birth of English Porcelain, Trans. A. C. S., Vol. III, page 144.

TABLE I.

No.	Chemical Formulæ				Locality
1	.0467 K_2O	1. Al_2O_3	5.976	SiO_2	Japanese Body
2	.1140 K_2O	1. Al_2O_3	4.168	SiO_2	Society of Hal. Belgium
3	.1160 K_2O	1. Al_2O_3	3.75	SiO_2	Plymouth Body
4	.1213 K_2O	1. Al_2O_3	4.82	SiO_2	Lahalleur Vierzon (Heavy)
5	.0707 K_2O .0860 CaO .1567 RO	1. Al_2O_3	3.22	SiO_2	Bristol Body
6	.1622 K_2O	1. Al_2O_3	3.95	SiO_2	Limoges (Fine)
7	.1836 K_2O	1. Al_2O_3	3.98	SiO_2	Limoges (Common)
8	.2020 K_2O	1. Al_2O_3	4.74	SiO_2	Limoges (Figures)
9	.1336 K_2O .0961 CaO .2297 RO	1. Al_2O_3	4.66	SiO_2	Karlsbad Body
10	.2400 K_2O	1. Al_2O_3	5.2	SiO_2	American Body
11	.2450 K_2O	1. Al_2O_3	5.027	SiO_2	Limoges Body
12	.2540 K_2O	1. Al_2O_3	4.51	SiO_2	Limoges (Fine)
13	.2584 K_2O	1. Al_2O_3	5.44	SiO_2	American Body
14	.2770 K_2O	1. Al_2O_3	7.06	SiO_2	Japanese Body
15	.2700 K_2O .0100 CaO .2800 RO	1. Al_2O_3	3.95	SiO_2	Limoges Body (Common)
16	.1635 K_2O .1186 CaO .2821 RO	1. Al_2O_3	4.35	SiO_2	Karlsbad Body
17	.2860 K_2O	1. Al_2O_3	6.05	SiO_2	Berlin Body
18	.3312 K_2O	1. Al_2O_3	4.74	SiO_2	Limoges Body
19	.3330 K_2O .0200 CaO .3530 RO	1. Al_2O_3	4.72	SiO_2	Chinese Body
20	.1290 K_2O .2070 CaO .3360 RO	1. Al_2O_3	4.89	SiO_2	Sevres Body

TABLE I—Continued.

No.	Chemical Formula			Locality
21	.1467 K_2O .2017 CaO .3484 RO	1. Al_2O_3	4.07 SiO_2	American Porcelain (Heavy)
22	.3583 K_2O .0014 CaO .0030 PbO .3627 RO	1. Al_2O_3	3.96 SiO_2	English Porcelain
23	.1590 K_2O .2160 CaO .3750 RO	1. Al_2O_3	4.33 SiO_2	American Porcelain (Fine)
24	.2880 K_2O .1255 CaO .4135 RO	1. Al_2O_3	3.7 SiO_2	Limoges Body (Fine)
25	.1776 K_2O .2442 CaO .4218 RO	1. Al_2O_3	4.16 SiO_2	American Body (Medium)
26	.1910 K_2O .2645 CaO .4555 RO	1. Al_2O_3	3.74 SiO_2	American Body (Medium)
27	.4360 K_2O	1. Al_2O_3	4.81 SiO_2	American Body (Fine)
28	.4900 K_2O	1. Al_2O_3	4.0 SiO_2	American Body (Medium)
29	.0960 K_2O .4040 CaO .5000 RO	1. Al_2O_3	3.08 SiO_2	Sevres Body (Heavy)
30	.2240 K_2O .3010 CaO .5250 RO	1. Al_2O_3	5.34 SiO_2	Hache and Pepin (Fine)
31	.0800 K_2O 1.7560 CaO 1.8360 RO	1. Al_2O_3	3.2 SiO_2	English Bone China
32	.2580 K_2O 2.0780 CaO 2.3360 RO	1. Al_2O_3	15.2 SiO_2	Dr. Wall's Body*
33	.2690 K_2O 2.5400 CaO 2.8090 RO	1. Al_2O_3	3.39 SiO_2 1.67 P_2O_5	Modern English Body*

*Trans. A. C. S., Vol. III, Birth of English Porcelain.

TABLE I—Continued.

No.	Chemical Formulæ			Locality
34	.1370 K_2O 2.7370 CaO 2.8740 RO	1. Al_2O_3	4.25 SiO_2 1.09 P_2O_5	Bow Body*
35	.2300 K_2O 5.2000 CaO 5.4300 RO	1. Al_2O_3	7.12 SiO_2 1.5 P_2O_5	Chelsea Body*
36	2.9350 K_2O 3.9040 Na_2O 5.2770 CaO 12.1160 RO	1. Al_2O_3	31.43 SiO_2	Dr. Wall's Body*

At first sight this list seems a hopeless conglomeration, ranging to all extremes, but closer study discloses the fact that, while the RO varies from .0467 in the Japanese to 5.43 in the old English, and from .114 in modern Belgium to 1.84 in modern English ware, the proportion of SiO_2 is comparatively constant. Only in two out of thirty-six cases does the SiO_2 run below 3.7, and in only two cases of modern porcelain does it run over 6.0 SiO_2. The average of the list I find to be between 4. SiO_2 and 4 4 SiO_2, hence I took 4.2 SiO_2 as the standard.

I next chose a standard from the RO. To obtain this by the same method as pursued with the SiO_2 would prove unsatisfactory, since to take an average of the entire list would result in a RO of 1.00, while to take an average of the thirty modern porcelains would give us about 0.3 RO. The first case would give us about a cone 6 product while the second case gives us very nearly the type of an American porcelain maturing at cone 12.

I did not think a choice between such extremes could be satisfactory, so I concluded to make a series covering each extreme and a third about half way between. To this end I started with the three following types:

Series A.	1. RO,	1. Al_2O_3,	4.2 SiO_2 — Cone 6 type
Series B.	.5 RO,	1. Al_2O_3,	4.2 SiO_2 — Cone 9 type
Series C.	.3 RO,	1. Al_2O_3,	4.2 SiO_2. — Cone 12 type

*Trans. A. C. S., Vol. III, Birth of English Porcelain.

Method of Production of the Trial Bodies.

The various porcelains I produced in the following manner:

The two extremes of each set, I weighed out and ground wet in a ball mill. The intermediate numbers I obtained by liquid blending of the extremes. After the blends were thoroughly mixed, I poured them into plaster molds to harden by loss of their water. Of each porcelain body I made the following ware:

2 brickettes made in standard cement,—brickette molds, and having a central cross section of 1 inch in the green state. These were made by the plastic process.

2 cubes of 50 grams weight made also by the plastic process.

2 tiles, 4½ inches x 4 ½ inches, in green state. These were made by the dry-press process.

The material for the dry pressed tile was prepared as follows: The dry body was thoroughly pulverized and was then dampened with water by means of a brush and the hand, thus obtaining a fairly uniform dampness. The damp clay was allowed to stand about two hours and was then put through a twenty-mesh screen and promptly pressed into the desired shape by means of a hand tile press. About 12 to 14 per cent. of water was in the clay when it was pressed.

Upon the various forms made up, I made the following tests:

Electrical resistance test, tensile test, abrasion test, absorption test and shrinkage test.

Preliminary Studies on the Electrical Test.

While no great obstacle lay in the way of the tensile, abrasion, absorption or shrinkage tests, I found a very great one confronting me, when I considered testing the electrical resisting properties of the porcelains. In the first place, I had no idea how much electrical resistance a given thickness of porcelain might display. In the second place, I could only press a tile of one size, viz: 4½ inches x 4½ inches. I saw that I must obtain, if possible, some insulating material in which to imbed the tile and thus

increase the arcing distance of the current, since with 4½ inches arcing distance, I could not use quite 55000 volts without the current arcing around the tile.

In searching for a material to use for the insulating apron referred to, I found that S. P. Thompson, in his treatise. "Electricity and Magnetism," page 310, gives the following table of specific resistances:

TABLE II.

Silver	100.
Copper	96.
Gold	74.
Iron Soft	16.
Dead	8.
German Silver	7.5
Mercury (liquid)	1.6
Glass at 200° C less than	.000,000,001
Gutta percha ——— less than	.000,000,001
Selenium Annealed less than	.000,000,0025
Pure Water at 22° C less than	.000,001

The preceding table of conductivity or resistance, as you may please to call it, indicates that the last four substances: Glass, gutta percha, selenium and pure water are far better insulators than any other substances ready at hand. The idea of using Portland cement, however, suggested itself, and as it seemed plausable that this might be a good insulating material, I proceeded to try it.

To do this, I took a semi-vitrified tile, 4 x 4 x ⅜ inches, and incased it in a slab of Portland cement (Dyckerhoff). The cement was carefully tamped around the tile so that a good attachment might result. I also made several other slabs of cement, both German and American. In these I encased no tiles. All the cement slabs stood six days in water, and were then taken out and carefully dried. I then proceeded to test their resisting properties with poor results. Each and every slab showed only so much electrical resistance as a like thickness of pure air would show, thus proving that the small insulating power displayed by the cement was due to the fact that its thickness held the electrodes apart and made an arc necessary.

I next tested the tile encased in cement. I had also provided myself with the following insulating materials: One sheet window glass; one sheet plate glass; one sheet plate glass; one porcelain tea plate; one tile 4x4x⅜ inches unincased. The results of the test of the above was as follows:

Tile 4x4x⅜ inches, incased in slab of Portland cement, punctured at .... 17,000 volts.
Tile 4x4x⅜ inches, unincased, punctured ..... 20,250 "
Sheet window glass 6x6x⅛ punctured at..... 40,000 "
Porcelain Tea Plate 6 inches in diameter x ⅛, punctured at..... .... 49,500 "
Sheet Plate Glass 6x6x¼ stood 60,000 volts and then the current arced around sheet and could not puncture it.

I would state here that the tile incased in cement and the unincased tile were both of the same composition, and as both were porous to a degree, it is to be presumed that the incased tile which had been soaked for six days still retained some moisture when tested, which accounts for its lower puncturing point than was displayed by the unincased tile. The facts brought out by this preliminary test seem about as follows:

First. That Portland cement does not possess any more insulating power than would be displayed by a like thickness of dry air.

Second. That porcelain, if not perfectly vitrified, does not possess any great insulating efficiency, even when perfectly dry.

Third. That glass ranks next and lies between semi-vitrified and vitrified porcelain.

Fourth. That thoroughly vitrified porcelain, even though only ⅛ inch thick, possesses as much insulating strength as is necessary, since 40,000 volts is the highest voltage that can be transmitted without arcing around a 4½ x 4½ inch tile.

Fifth. That plate glass possesses an especially high electrical resisting property.

We know that the composition of glasses has a great influence on their ability to resist electric currents. Also, that the temperature at which they are used is a factor.

The following data were obtained from Widemann on "Electricity," Vol. 1, page 560:

TABLE III.

HEAVY LEAD GLASS		WHITE MIRROR GLASS		GREEN BOTTLE GLASS	
Analysis		*Analysis*		*Analysis*	
K_2O..... 4.0		K_2O..... 8.67		K_2O ... 6.53	
Na_2O... —		Na_2O . 9.48		Na_2O... 8.68	
PbO.... 67.0		CaO..... 16.00		CaO.... 12.55	
SiO_2... 20.0		SiO_2... .. 67.80		SiO_2.... 68.02	
100		96.88			
Melting point. 700°C.				Melting point, 1080°C.	
Temp.°C	Resistance.	Temp.°C	Resistance.	Temp.°C	Resistance.
216.	188600.	189.	182200.	170.5	173200.
226.	113160.	200.	109320.	180.4	86600.
234.	75440.	214.	54660.	196.8	34640.
249.	37720.	238.5	27330.	211.0	17320.
269.	18860.	247.	12754.	222.5	10392.
286.	11816.	269.	5466.	250.5	3464.
297.	7544.	299.	1822.	266.5	1732.
312.	3772.	316.5	1093.2	305.5	519.6
326.	1886.	329.	728.8	327.5	259.8
341.	943.	349.	364.4	343.0	173.2

The foregoing tables take mercury as the unit of conductivity, or as having a resistance of 1.

To return to my search for an aproning material to use with my tiles, I found that a gutta-percha plate could not be cemented to the tile for each test and then removed, and to furnish a separate sheet of gutta-percha for each tile would mean entirely too great an expense. Selenium also was out of the question owing to its cost. I concluded, however, to try sulphur, which is a very similar material to selenium, and to do this I took two bottomless flasks and by means of sulphur in the melted state I cemented these two flasks to two tiles. I also cast two large plates of sulphur with tiles imbedded in their centers. One tile was completely imbedded leaving only a hole ½ inch in diameter on each side to admit the electrode. The second tile was only imbedded on one side in the sulphur, the other side was left exposed. There was a one half inch hole on the imbedded

side of the tile to admit the eloctrode. The tiles were vitrified. I also cast a plate of sulphur 6x6x½ inches. The results of the electrical test on sulphur was as follows:

The plate containing the half imbedded slab stood about 58.000 volts and then the current arced through the sulphur. The plate containing the completely imbedded tile stood about the same, the current going around the tile through the sulphur. The tiles cemented to the flasks were not satisfactory since the current found its way between the glass and the sulphur. The slab of pure sulphur ½ inch thick stood about 13.500 volts and then punctured. On its being punctured the slab caught fire from the spark.

I felt satisfied that at least none of the materials available could be successfully used as an aproning material so I discarded the idea of aproning my tiles, and reduced the thickness of the pieces to be tested electrically to an amount which would puncture before the current would arc around the tile.

Formulæ of the Porcelain Bodies Made.

I have already given the formulæ of the three porcelains which I selected as fairly illustrating the types now in actual use. Taking each type as a basis, I made three separate sub-series of bodies as follows:

I. Maintaining RO and SiO_2 constant and varying the proportion of Al_2O_3.

II. Maintaining the Al_2O_3 to SiO_2 constant and varying the RO in kind but not in quantity.

III. Maintaining the RO and Al_2O_3 constant and varying the proportion of SiO_2.

Each sub-series consisted of a set of five porcelains. In this way, I expected to cover the ground most completely.

Following I give the detailed chemical formulæ of the various porcelains included in this investigation:

SERIES A—CONE 6 TYPE.

In Series A I use Dr. Seger's RO, viz.: $\left.\begin{matrix} .3\ K_2O \\ .7\ CaO \end{matrix}\right\}$

Sub-Series A-I.

A-I-1	.3 K_2O / .7 CaO	.7 Al_2O_3 4.2 SiO_2
A-I-2	.3 K_2O / .7 CaO	.8 Al_2O_3 4.2 SiO_2
A-I-3	.3 K_2O / .7 CaO	.9 Al_2O_3 4.2 SiO_2
A-I-4	.3 K_2O / .7 CaO	1.0 Al_2O_3 4.2 SiO_2
A-I-5	.3 K_2O / .7 CaO	1.1 Al_2O_3 4.2 SiO_2

*Series A-II.**

A-II-1	.3 K_2O / .7 CaO	1. Al_2O_3 4.2 SiO_2
A-II-2	.4 K_2O / .6 CaO	1. Al_2O_3 4.35 SiO_2
A-II-3	.5 K_2O / .5 CaO	1. Al_2O_3 4.5 SiO_2
A-II-4	.6 K_2O / .4 CaO	1. Al_2O_3 4.65 SiO_2
A-II-5	.7 K_2O / .3 CaO	1. Al_2O_3 4.8 SiO_2

Series A-III.

A-III-1	.3 K_2O / .7 CaO	1. Al_2O_3 3.2 SiO_2
A-III-2	.3 K_2O / .7 CaO	1. Al_2O_3 4.2 SiO_2
A-III-3	.3 K_2O / .7 CaO	1. Al_2O_3 5.2 SiO_2
A-III-4	.3 K_2O / .7 CaO	1. Al_2O_3 6.2 SiO_2
A-III-5	.3 K_2O / .7 CaO	1. Al_2O_3 7.2 SiO_2

SERIES B — CONE 9 TYPE.

Series B-I.

B-I-1	.8 K_2O / .2 CaO	.7 Al_2O_3 4.2 SiO_2
B-I-2	.8 K_2O / .2 CaO	.8 Al_2O_3 4.2 SiO_2

* In Series A-II it was necessary to vary the SiO_2 content slightly in order to introduce the additional K_2O as feldspar.

B-I-3	.3 K_2O .2 CaO	.9 Al_2O_3 4.2 SiO_2
B-I-4	.3 K_2O .2 CaO	1.0 Al_2O_3 4.2 SiO_2
B-I-5	.3 K_2O .2 CaO	1.1 Al_2O_3 4.2 SiO_2

Series B-II.

B-II-1	.5 CaO	1. Al_2O_3 4.2 SiO_2
B-II-2	.375 CaO .125 K_2O	1. Al_2O_3 4.2 SiO_2
B-II-3	.25 CaO .25 K_2O	1. Al_2O_3 4.2 SiO_2
B-II-4	.125 CaO .375 K_2O	1. Al_2O_3 4.2 SiO_2
B-II-5	.5 K_2O	1. Al_2O_3 4.2 SiO_2

Series B-III.

B-III-1	.3 K_2O .2 CaO	1. Al_2O_3 3.2 SiO_2
B-III-2	.3 K_2O .2 CaO	1. Al_2O_3 4.2 SiO_2
B-III-3	.3 K_2O .2 CaO	1. Al_2O_3 5.2 SiO_2
B-III-4	.3 K_2O .2 CaO	1. Al_2O_3 6.2 SiO_2
B-III-5	.3 K_2O .2 CaO	1. Al_2O_3 7.2 SiO_2

SERIES C—CONE 12 TYPE.

Series C-I.

C-I-1	.2 K_2O .1 CaO	.7 Al_2O_3 4.2 SiO_2
C-I-2	.2 K_2O .1 CaO	.8 Al_2O_3 4.2 SiO_2
C-I-3	.2 K_2O .1 CaO	.9 Al_2O_3 4.2 SiO_2
C-I-4	.2 K_2O .1 CaO	1.0 Al_2O_3 4.2 SiO_2
C-I-5	.2 K_2O .1 CaO	1.1 Al_2O_3 4.2 SiO_2

Series C-II.

C-II-1	.3 CaO	1. Al_2O_3 4.2 SiO_2
C-II-2	.225 CaO, .075 K_2O	1. Al_2O_3 4.2 SiO_2
C-II-3	.15 CaO, .15 K_2O	1. Al_2O_3 4.2 SiO_2
C-II-4	.075 CaO, .225 K_2O	1. Al_2O_3 4.2 SiO_2
C-II-5	.3 K_2O	1. Al_2O_3 4.2 SiO_2

NOTE—Owing to a lack of time, no attempt was made to run a C-III Series, but an approximate idea on the variation of silica at this temperature will be obtained by a study of Series C-I.

PRELIMINARY TEST.

I was unwilling to spend the great amount of time and labor on this investigation, without some assurance that the mixtures I proposed to make, as shown by the preceding formulæ, would give me the range of workable porcelains which I expected. I therefore made cones of all the various porcelain bodies to be tested and each series was tested against a standard Seger cone of the temperature to which that series was to be fired. The results of this test were as follows:

Series A—tested against Cone 6.

Specimen A-I-1 shows about the same fusing point as cone 5, hence is useless at this temperature.

Specimen A-I-2 developed into a splendid porcelain of high grade. Translucency is at its maximum, but indications of fusion are apparent.

Specimen A-I-3 is a splendid porcelain with a slight trace of stoniness and good translucency.

Specimen A-I-4 gives a perfectly stony porcelain, but perfectly vitrified and with slight translucency.

Specimen A-I-5 evidently too high in Al_2O_3 for this temperature. Has a dead white surface with no translucency and evidence of porosity.

Specimen A-II-1 is the same as A-I-3 and shows a very similar development.

Specimen A-II-2 is somewhat further developed than A-II-1, but only shows translucency on the edges.

Specimen A-II-3 begins to take on a characteristic porcelain appearance, quite translucent with slight stoniness.

Specimen A-II-4 has about the same degree of development as displayed in A-I-3, good translucency with only slight stoniness.

Specimen A-II-5. We here reach again the high grade porcelain type, all traces of stoniness having disappeared and the surface being self-glazed, as in A-I-2.

Specimen A-III-1 shows a hard dead white surface with no signs of translucency and with indications of porosity.

Specimen A-III-2 same as A-I-4 and A-II-1.

Specimen A-III-3. Slightly improved over A-III-2 in translucency but otherwise unchanged.

Specimen A-III-4 shows fair translucency but is too high grade a porcelain for much strength.

Specimen A-III-5. Good translucency but is semi-glazed, indicating over maturity.

Conclusions on Appearance of Series A.

Series A-I seems to indicate that at this temperature we may expect good porcelains with from .8 to 1.0 Al_2O_3 Below .8 Al_2O_3, we find the body melts out of shape and above 1. Al_2O_3 it does not mature. .8 or .9 Al_2O_3 gives a fine translucent porcelain at cone 6.

Series A-II shows that CaO alone does not produce a porcelain and apparently does not flux materially at this temperature. Evidently, CaO does not produce any translucency, but changes suddenly from the dry to the glassy state. K_2O seems to have done its maximum amount of fluxing, since A-II-5 shows a splendid degree of vitrification and translucency.

Series A-III seems to indicate that an increase of SiO_2 does not affect the appearance of the porcelain to the same degree as do the other two constituents. However, it shows that to obtain a good porcelain, the SiO_2 should be between 4.2 and 6.2 SiO_2, as below 4.2 SiO_2 we get immaturity and above 6.2 SiO_2 we get a weak glassy product.

Series B. Tested Against Cone 9.

Specimen B-I-1 shows about the same degree of development as cone 9, so that its absolute fusing point is too nearly reached to make it a practical porcelain at this temperature.

Specimen B-I-2 is a brilliant, self-glazed porcelain, and has a fine translucence.

Specimen B-I-3 shows a fair degree of translucency and a tendency to stoniness.

Specimen B-I-4 is a stony appearing porcelain with only traces of translucency on edges.

Specimen B-I-5 is a very stony body with a little or no trace of translucency.

Specimen B-II-1 could not properly be called a porcelain. It is a porous, cream-white biscuit,

Specimen B-II-2 displays a marked improvement over B-II-1 in that it is a closer, stonier body, but signs of porosity are still noted.

Specimen B-II-3 seems to be a good stony porcelain, and from all appearances is perfectly vitrified. It displays traces of translucency on all exposed points and has a smooth satiny surface.

Specimen B-II-4 is a fine porcelain, with good translucency and a smooth surface. It still retains some signs of stoniness.

Specimen B-II-5 gives the finest porcelain so far obtained. Perfect translucency without a trace of stoniness and brilliantly self-glazed on all exposed points.

Specimen B-III-1 shows a slightly undermatured porcelain but does not appear porous.

Specimen B-III-2. Same as B-I-4, and shows a similar development.

Specimen B-III-3 displays a fair translucency with traces of stoniness.

Specimen B-III-4 is a high grade porcelain with maximum translucency and a self-glazed surface.

Specimen B-III-5 shows signs of overmaturity. It has an opaque glassy appearance on the surface which destroys translucency.

Conclusions on Appearance of Series B.

Series B-I seems to indicate that we may expect good porcelain with .8 to 1.0 Al_2O_3, the RO and SiO_2 being as shown in this series.

Series B-II appears to show that we may not expect to obtain porcelain at this temperature unless we use at least one-half the RO as alkali. Above this amount we get good porcelains in every case, even where we use K_2O alone.

Series B-III confirms the conclusion in A-III that the variation of SiO_2 does not vary the appearance of the product so much as a variation of either of the other constituents. However, 3.2 SiO_2 seems insufficient, while 7.2 SiO_2 in undoubtedly an excess.

Series C. Tested Against Cone 12.

Specimen C-I-1 shows a maximum maturity. Very translucent and brilliantly self-glazed.

Specimen C-I-2 is a splendid porcelain, fine translucency and a good smooth surface.

Specimen C-I-3 shows a fair degree of translucency, with traces of stoniness.

Specimen C-I-4. Slightly translucent, marked stoniness.

Specimen C-I-5. Somewhat undermatured and little, if any, translucency.

Specimen C-II-1 is a good hard biscuit, but without any signs of translucency.

Specimen C-II-2 is a hard body, but still lacks any traces of translucency.

Specimen C-II-3 gives a good stony porcelain, with traces of translucency on exposed points.

Specimen C-II-4 is a fine translucent porcelain.

Specimen C-II-5 shows a very strongly translucent porcelain with an extremely glassy surface.

Conclusions on Appearance of Series C.

Series C-I shows us that undoubtedly a good porcelain at this temperature must contain, at least 0.8 Al_2O_3 and not over 1.0 Al_2O_3.

Series C-II indicates that even at this temperature we get no translucency from CaO. With all the RO as K_2O, we get a fine appearing body with maximum translucency.

We conclude from the previous tests and from Series C-I that Series C-III would give us a graded increase from stoniness to glassiness.

PREPARATION OF THE REGULAR TRIAL PIECES.

The preliminary testing having been completed, and having reassured me of the suitability of the series as projected to bring out the facts for which we were seeking, I then began the main work. The various bodies were prepared on sufficient scale, and were worked into the shapes heretofore decided upon and after drying each lot thoroughly, I fired them. It was impossible to obtain in the small kiln used, an absolutely uniform temperature over the entire kiln. However, I think I can say, that except in one case, all received an equal period of firing. All the pieces were imbedded in silica sand, except Series A-III from which the sand leaked away to some extent during the firing process. Series A had to be refired, as it was not fully matured at the first firing. Of course in such a line of work as I have pursued, the factor of error due to a variation of the process is very great, but I have made an effort to work all the bodies as nearly alike as possible and think that even with these factors of error present, we may find some valuable facts by a study of the results of the tests.

REMARKS ON THE RESULTS OF SERIES A.

ELECTRICAL RESISTANCE

As I only had 4½"x4½" tile for this test, I could not expect to obtain above a test of 50,000 volts. For this reason I reduced the thickness of my tile at the center to about $\frac{3}{16}$ inch. The voltage for this test was obtained by means of a transformer provided for me by the Electrical Engineering Department of Ohio State University and operated by R. D. DeWolf, fellow in that department. Our power was obtained from the power circuit of O. S. U.

The test resulted as follows:

Series A-I—See Table

A-I-1 was melted out of shape and could not be tested. A-I-2 and A-I-3 stood the maximum voltage, arcing at 47,250 volts without puncturing. A-I-4 and A-I-5 both show a decreasing efficiency as insulators.

Of this group, I think A-I-3 seems the most desirable.

Series A-II—See Table

This series gives apparent confirmation to my opinion that a glassy porcelain has not the insulating strength of a semi-stony one.

A-II-1 shows a poor insulating power, due to imperfect vitrification.

A-II-2 seems improved in vitrification and shows a higher resistance.

A-II-3, which proved the best in the tensile test, again seems preferable, standing the maximum voltage applied.

A-II-4 stands the same voltage without puncturing, as did A-II-3, but its inferiority as shown in the tensile test makes it less desirable than the preceding specimen.

A-II-5 failed to stand up under the voltage applied and punctured at a maximum of 36,900 volts.

This, with its indicated weakness in tensile strength, would lead us to believe that we must look elsewhere than to pure feldspar porcelains for the highest insulating materials.

Series A-III—See Table

This series shows an increasing electrical resistence with an increasing proportion of SiO_2.

A-III-1 could not be worked in a dry press process due to the high clay contents. All the tiles cracked in burning, hence no electrical test was obtained.

A-III-2 punctured at 13,050 and 10,250 volts, respectively.

A-III-3 was only tested in $\frac{1}{2}$ inch thickness, as all the $\frac{3}{16}$ inch specimens were imperfect. At $\frac{1}{2}$ inch thickness, this specimen stood 39,400 volts without puncturing, the current arcing around the tile at this voltage.

A-III-4 at $\frac{3}{16}$ inch thickness stood the maximum voltage without puncturing and arced at 49,500 volts. A $\frac{3}{32}$ inch tile punctured at 38,250 volts.

A-III-5 at $\frac{3}{16}$ inch thick could not be punctured but arced around at 49,500 volts. However, the specimens all show over-maturity, which makes this composition an undesirable one.

I should consider A-III-4 as the best of this series.

ABRASION TEST.

The abrasion test was made by filling a large ball mill with two hundred pounds of flint pebbles and enough water to cover them completely. The brickettes were then weighed and placed in the mill, which was then closed and started. Two separate sets of tests were made upon each type of porcelain. One set was subjected to a test of 1000 revolutions and the other was subjected to a 2000 revolution test. The results are as follows:

Series A-I— See Table

A-I-1 seems to have either been softened and weakened by over-maturity, or to be naturally of a weak, fragile composition, due to lack of Al_2O_3 in its composition. The specimens of this type both wore badly, although they did not chip.

A-I-2 seems to have stood the test remarkably in one case, and broken down badly in the other case.

A-I-3 stood the 1000 revolution test and showed no signs of wear. It wore badly in the 2000 revolution test, however.

A-I-4 stood the 1000 revolution test well, but lost 2.03 per cent. on the 2000 revolution test.

A-I-5 lost 1.02 per cent. in the 1000 revolution test, indicating a uniform density and maturity throughout.

From the evidence at hand, A-I-4 is undoubtedly the choice of this series.

Series A-II— See Table

A-II-1, 2 and 3 can not be very satisfactorily compared from the data obtained, since one of each type was broken in the test and lost heavily from chipping.

A-II-4 and 5, both stood the tests without breaking in either case, but both lost considerably by wear.

Of this, Series A-II-4 seems the most desirable.

Series A-III— See Table

A-III-1 wore badly in both cases.

A-III-2 and 3 stood perfectly in the 1000 revolution test, but both broke in the 2000 revolution test.

A-III-5 loses nothing in either test. Hence, we must accept A-III-5 as the best as indicated from this test.

ABSORPTION TEST.

We only find three specimens of this series that are not completely vitrified. These are, A-I-5 and A-III-1 and 2. I cannot explain the cause of A-III-2 being soft. I think some error has been made in compounding this specimen, since A-III-2 seems at fault whenever tested, while A-I-2 and A-II-1, which are of the same composition, show no lack of vitrification.

TENSILE STRENGTH.

This test was made on a standard machine, using pasteboard around the ends of the brickettes to act as cushions and prevent cracking from direct contact with the jaws of the machine which were plain and not fitted with rolls as are the jaws of machines now made for testing cement brickettes. The brickettes when made had a central cross section of one square inch. This was reduced by shrinkage in drying and firing so that it was necessary to calculate the tensile strength per square inch in each case. The fractures resulting from this test proved very interesting and I believe very instructive. They are as follows:

Series A-I— See Tables

A-I-1 shows a glassy fracture, somewhat bloated and very open.

A-I-2 shows a very glassy fracture, but not so badly bloated as A-I-1.

A-I-3 has a fairly dense semi-glassy plane of fracture with little or no signs of bloating.

A-I-4 shows a dense porcelain fracture, somewhat stony in appearance.

A-I-5 has a dull grey fracture plane which shows traces of immaturity and porosity. A-I-5*a* showed a weak center which was yellow and apparently under-matured.

Of Series A-I, the strongest is either A-I-3 or A-I-4. A-I-3 shows considerable strength, but the bloating indicated over-maturity. A-I-5 shows weakness from immaturity, as might be expected.

Series A-II—See Tables

A-II-1 being the same as A-I-4 in composition, has much the same general appearance.

A-II-2 shows a slight increase in glossiness and a smoother fracture.

A-II-3 has a glossy fracture but is very dense. A-II-3b has large interior flaw which weakens it.

A-II-4 has a very glossy fracture which discloses minute cavities as evidence that bloating has begun.

A-II-5 displays a very badly bloated fracture full of large cavities.

Of the A-II series the test shows A-II-3 to be the best. This specimen has a composition of

$$\left.\begin{matrix}5\,K_2O\\5\,CaO\end{matrix}\right\}\,1.Al_2O_3\left\{4.2\,SiO_2\right.$$

which seems to indicate that where calcium is much in excess, we do not get the degree of density unless it be just on the verge of the fusing period. Without density we cannot have strength. At the same time the test seems to condemn A-I-5 as being brittle from excess of K_2O which caused glassiness.

Series A-III—See Table

A-III-1 shows an under-matured creamy fracture with evidence of considerable porosity. A-III-1a has a flaw in the line of fracture, hence its weakness.

A-III-2 shows a somewhat denser stonier fracture and but little sign of glassiness. A-III-2a has a flaw along its plane of fracture.

A-III-3 does not show a very much denser structure than does A-III-2. I can not explain this.

A-III-4 shows a glossy surface, perfect vitrification and a close dense structure.

A-III-5 is completely vitrified but is glassy and shows traces of bloating.

Of this group I think A-III-4 is the best. This series tends to confirm the indication of series A-1 that increased SiO_2 brings density, since the density appears to steadily increase up to A-III-5, but this later shows signs of bloating which means that its limit of endurance is nearly reached.

SHRINKAGE TEST.

A study of the results of this test indicates that the specimen having the greatest shrinkage stands the most severe test. This is not always the most highly vitrified, but oftener the highest in clay substance.

In the discussion of this series, I would say that I think much more satisfactory results would have been obtained, had

$$\left.\begin{matrix} .5\ K_2O \\ .5\ CaO \end{matrix}\right\} \text{ been used instead of } \left\{\begin{matrix} .3\ K_2O. \\ .7\ CaO. \end{matrix}\right.$$

as a standard type.

The choice of this series lies between A-I-3 and 4, A-II-4 and A-III-3 and 4.

REMARKS ON THE RESULTS OF SERIES B.

We will now take up the results of Series B tests, which were made in the same manner as were those of Series A.

ELECTRICAL RESISTANCE

Series B-I—See Table

B-I-1 and 2 are both excellent insulators, standing 47,850 volts and arcing around. Neither were punctured.

B-I-3, 4 and 5 all broke at comparatively low voltages, indicating either immaturity or lack of insulating efficiency.

Of this series, I think B-I-2 is decidedly the best, as B-I-1 has too narrow a limit of fusibility after maturity is reached.

Series B-II—See Table

B-II-1 showed a decided weakness, due to lack of density. The flux here was CaO alone.

B-II-2 shows some improvement from the addition of a small amount of K_2O.

B-II-3 and 4 appear no better than the preceding so far as efficiency is concerned.

B-II-5 shows a slight improvement.

I think this entire series is too high in Al_2O_3 for this temperature, at least when made by the dry press process.

Series B-III—See Table

B-III-1 could not be tested, as every specimen cracked in burning, owing to an excess of clay.

B-III-2 shows a moderate degree of development as an insulator.

B-III-3 shows that a maximum of 49,500 volts is insufficient to puncture three-sixteenths inch.

B-III-4 and 5 also stand 49,500 volts, and then arc around without puncturing.

Of this series, the last three B-III-3, 4 and 5, all show greatly increased insulating power over the other numbers of the series. Of these, B-III-3 or 4 are both very superior porcelains, B-III-5 showing a slight over-maturity.

ABRASION TEST.

Series B-I—See Table

B-I-1 shows a nearly equal loss in both the 1000 and the 2000 revolution test.

B-I-2 shows an equally good body, standing both tests without chipping.

B-I-3 is somewhat more severely attacked, but even in the 2000 revolution test does not lose two per cent.

B-I-4 breaks into pieces in the 1000 revolution test, but stands the 2000 test without breaking. (The first case must have been a defective brickette.)

B-I-5 breaks both in the 1000 and 2000 revolution test.

Of this series I think B-I-2 and 3 are both good bodies.

Series B-II—See Table

B-II-1 wears very badly owing to its high CaO content and low degree of vitrification.

B-II-2 shows a decided improvement over the B-II-1 specimen.

B-II-3 loses nothing in the 1000 revolution test.

B-II-4 shows its increasing glassiness, due to its increasing K_2O content. It breaks in the 2000 revolution test and loses 10 per cent.

B-II-5 loses 10 per cent. in the 1000 revolution test and over 12 per cent. in the 2000 revolution test.

I think we are safe in selecting B-II-3 as the choice of this series.

Series B-III—See Table

B-III-1 loses about the average one per cent. in the 1000 revolution test and two per cent. in the 2000 revolution test.

B-III-2 broke in the 1000 test and lost 4.4 per cent. In the 2000 test it loses only 2.27 per cent.

B-III-3 broke in the 2000 revolution test, but stood the best test of any in the 1000 revolution test.

B-III-4 lost three per cent. and B-III-5 lost 3.45 per cent. by actual abrasion in the 2000 revolution test.

I think we may safely select B-III-3 as the best of this series.

ABSORPTION TEST.

The absorption test on series B shows only one specimen to be at all porous, viz.: B-II-1, in which CaO alone was used as a flux.

TENSILE TEST.

Series B-I—See Table

B-I-1 shows a porous, glassy fracture.

B-I-2 shows a much denser, but still a glassy fracture.

B-I-3 shows some signs of stoniness with a dense dull fracture.

B-I-4 shows a decidedly stony fracture, with a flinty appearing surface.

B-I-5 displays increased stoniness.

The tensile tests favor specimen B-I-4 very much, and since it stands 2449 pounds per square inch, in one specimen having an internal flaw, I think we may say that from this test we choose B-I-4 as the best.

Series B-II— See Table

In B-II series we see again, as in series A-II, the gradation caused by replacement of CaO by K_2O as a flux.

B-II-1 shows only a dense biscuit-like body with slight porosity.

B-II-2 has lost its porosity, but still lacks any trace of translucency and displays a purely clayey fracture.

B-II-3 give a splendid stony porcelain, with a dense smooth plane of fracture.

B-II-4 shows a glassier fracture plane and an appearance of brittleness.

B-II-5 has a brilliant fracture surface and indicates that it is decidedly brittle.

In this series the test shows very clearly that when more than 50 per cent. of the RO is K_2O, the body begins to deteriorate from brittleness, while with over 50 per cent. of the RO in the form of CaO, we note immaturity or lack of vitrification.

Series B-III— See Table

Series B-III shows an increasing strength with an increasing silica content.

B-III-1 shows a dull, stony plane of fracture.

B-III-2 shows an improvement in the appearance of the fracture over that noted in the preceding.

B-III-3 shows a very fair porcelain-like break with slight glassiness.

B-III-4 shows the same break as noted in fine high grade porcelains, but somewhat less translucency is apparent.

B-III-5 shows some signs of open structure due to overmaturity.

Of this, Series B-III-4 seems the most promising, with B-III-3 a close second.

SHRINKAGE TEST.

The shrinkage test on series B seemed to show little that we did not already know. The specimens high in K_2O maintain their size uniform with the rest of the series. Where two specimens of the same composition are unequally

matured, the specimen showing the greatest shrinkage invariably stands the severest test.

I think we may safely select B-I-2 from the B-I series as showing the best average in all tests. In B-II series, we find B-II-3 leads the others. In B-III series, specimen B-III-3 is the favorite.

REMARKS ON THE RESULTS OF SERIES C.

ELECTRICAL RESISTANCE.

Note that in series C the plates were only ⅛ inch in thickness, instead of 3-16 inch as in previous tests. A test is also recorded at ¼ inch thick.

C-I-1 stands the maximum, 34,000 volts at ⅛ inch thickness, and is not punctured at 47,500 at ¼ inch thick.

C-I-2 and 3 show a decreasing efficiency as insulators. C-I-4 is notably better than the two preceding in this respect, perhaps owing to its highly developed stony structure. The choice in this series is, however, C-I-1 or 2.

Series C-II shows an increasing electrical resistance with an increasing K_2O contents, but every specimen tested punctured, the best being the one of pure K_2O flux. This is contrary to the evident influences of K_2O alone as a flux according to the observations at lower temperatures. We must concede C-II-4 and 5 as the best of this series from the insulating efficiency evinced in this test.

ABRASION TEST.

Series C-I shows a magnificent record in this test. Both the 1000 and the 2000 revolution tests failed to in any way affect any of this series, and the blocks tested showed no evidence of having undergone any abrasive action whatever. I take it that this is a very strong argument in favor of high fire porcelain.

C-II-1 with CaO alone as a flux loses 8 to 12 per cent. by pure abrasion, showing that it lacks density and strength.

C-II-2 loses only 1.63 per cent. in the worst specimen, showing a marked improvement.

C-II-3 and 4 lose nothing in either case.

C-II-5 with K_2O alone as a flux loses 3.33 per cent. and 2.34 per cent. by chipping, which seems a characteristic of pure potash porcelain.

Of C-II series we may safely say that favorite is either C-II-3 or 4.

ABSORPTION TEST.

The test showed the entire series to be completely vitrified.

On the series C-II tests, specimen C-II-1 and 2 showed lack of perfect vitrification, the other members all proving perfectly vitrified.

TENSILE TEST.

Series C-I— See Table

C-I-1 shows a glassy fracture with a slight trace of over-maturity. That it is at its best is evinced by one specimen standing 2,100 pounds per square inch.

C-I-2 shows a fine close body with a good bright plane of fracture but no signs of over-maturity. One specimen stood 3071 pounds per square inch, the maximum strength displayed by any specimen in this work.

C-I-3 shows a good close structure and a tendency to stoniness.

C-I-4 is an extremely dense but a very stony porcelain.

C-I-5 shows an excess of clay in its composition as it has a dead and weak appearance to its fracture.

C-I-1 and 2 show the highest degree of tensile development of this series.

Series C-II— See Table

C-II-1 gives us only a good biscuit.

C-II-2 is a very hard biscuit, but lacking in vitrification and translucency.

C-II-3 has a close structure with good vitrification and dense stoniness.

C-II-4 is a good semi-stony porcelain with a splendid appearance of translucency.

C-II-5 is a fine well matured porcelain of a glassy nature.

The choice of the above as indicated by the tensile test would be C-II-4 or 5.

SHRINKAGE TEST.

The tests showed a slightly increasing shrinkage with an increase of Al_2O_3 from clay substance until C-I-3 is reached. Above this no increased shrinkage is apparent. In the C-II series, the increased K_2O seemed to cause increased shrinkage.

C-II-2 for some reason shrinks equally with C-II-4 and 5. My only explanation of this would be that the large amount of clay substance present in this specimen is enabled to act by the addition of the small amount of feldspar.

Why C-II-3, also, did not shrink proportionately I can not explain. I note, however, that in every case the first addition of K_2O seems to cause an increased shrinkage, not continuing to increase in the higher K_2O porcelains.

We may choose from Series C-I as favorites, either C-I-1 or 2. From Series C-II, I think C-II-4 is the best.

The data which have been discussed in the foregoing, will be found in tabular form in the following eight pages:

TESTS OF SERIES.

A–I

ELECTRICAL RESISTANCE TEST.

No.		Thickness	Puncturing Voltage	Remarks:
1	a	———	———	Melted out of shape and could not be tested.
	b	———	———	
2	a	3/16 inch.	Not punct'd	Arced around at 47,250 Volts.
	b	3/16 "	" "	" " at 47,250 "
3	a	3/16 "	" "	" " at 47,250 "
	b	—	—— ——	
4	a	3/16 "	14,400	
	b	3/16 "	13,500	
5	a	3/16 "	11,475	
	b	3/16 "	10,575	

ABRASION TEST
With 200 Pounds Flint Pebbles in Wet Ball-Mill

No.		Before Test	After Test	Per Cent. Loss
1	a	51.0 grs.	49.0 grs.	3.92
	b	49.0 "	47.0 "	4.08
2	a	52.5 "	52.0 "	0 95
	b	63.0 "	58.5 "	7.14
3	a	48.0 "	47.0 "	2.08
	b	48.7 "	48.5 "	0.51
4	a	49.2 "	48.2 "	2.03
	b	47.5 "	47.5 "	——
5	a	48.0 "	47.0 "	2.08
	b	49.0 "	48.5 "	1.02

a= 2000 revolution test.
b= 1000 " "

ABSORPTION TEST.

No.		Before Soaking	After Soaking	Per Cent. Absorpt'n
1	a	51.0 grs.	51.0 grs.	——
	b	49.0 "	49.0 "	——
2	a	52.5 "	52.0 "	——
	b	63.0 "	63.0 "	——
3	a	48.0 "	48.0 "	——
	b	48.7 "	48.7 "	——
4	a	49.2 "	49.2 "	——
	b	47.5 "	47.5 "	——
5	a	45.5 "	48.0 "	5.50
	b	48.5 "	49.0 "	1.08

TENSILE TEST.

No.		Area of Cross Section.	Load Neces'ary to Break	Tensile Strength of 1 Sq. Inch
1	a	11/16 x 11/16	912 lbs.	1390 lbs.
	b	— x —	—— "	—— "
2	a	11/16 x 11/16	1777 "	2408 "
	b	11/16 x 11/16	1653 "	2161 "
3	a	11/16 x 11/16	1882 "	2378 "
	b	11/16 x 11/16	1918 "	2505 "
4	a	11/16 x 11/16	1766 "	2391 "
	b	11/16 x 11/16	2088 "	2866 "
5	a	11/16 x 11/16	904 "	1181 "
	b	11/16 x 11/16	1566 "	2056 "

SHRINKAGE TEST.

No.		Area Green	Area Burned	Per Cent. Loss
1	a	1. Sq. In.	.656 sq. in	34.40
	b	— "	—— "	——
2	a	1. "	.738 "	26.20
	b	1. "	.764 "	23.54
3	a	1. "	.793 "	20.70
	b	1. "	.765 "	23.44
4	a	1. "	.738 "	26.17
	b	1. "	.711 "	28.90
5	a	1. "	.765 "	23.44
	b	1. "	.761 "	23.88

TESTS OF SERIES. (Continued)

A-II.

ELECTRICAL RESISTANCE TEST.

No.		Thickness	Puncturing Voltage	Remarks:
1	a	1/16 inch	10,700	
	b	— "	—	
2	a	1/16 "	23,625	
	b	1/16 "	36,000	
3	a	1/16 "	Not punct'd	Arced around at 47,250 Volts
	b	1/16 "	" "	" " " 47,250 "
4	a	1/16 "	" "	" " " 47,250 "
	b	1/16 "	" "	" " " 47,250 "
5	a	1/16 "	36,900	
	b	1/16 "	27,450	

ABRASION TEST
With 200 Pounds Pebbles in Wet Ball-Mill

No.		Before Test	After Test	Per Cent. Loss
1	a	40.0 grs.	38.0 grs.	5.00
	b	45.0 "	43.0 "	4.44
2	a	45.0 "	43.0 "	4.44
	b	45.2 "	44.5 "	1.67
3	a	49.5 "	46.5 "	6.05
	b	49.0 "	48.5 "	1.02
4	a	53.5 "	53.0 "	0.94
	b	45.5 "	45.0 "	1.10
5	a	55.0 "	54.0 "	1.81
	b	50.5 "	50.0 "	0.99

a = 2000 revolution test.
b = 1000 " "

ABSORPTION TEST.

No,		Before Soaking	After Soaking	Per Cent. Absorpt'n
1	a	40.0 grs.	40.0 grs.	——
	b	45.0 "	45.0 "	——
2	a	45.0 "	45.0 "	——
	b	45.2 "	45 2 "	——
3	a	49.5 "	49.5 "	——
	b	49.0 "	49.0 "	——
4	a	53.5 "	53.5 "	——
	b	45.5 "	45.5 "	——
5	a	55.0 "	55.0 "	——
	b	50.5 "	50.5 "	——

TENSILE TEST.

No.		Area of Cross Section	Load Neces'ary to Break	Tensile Strength of 1 Sq. Inch
1	a	19/32 x 1/4	887 lbs.	1118 lbs.
	b	1/16 x 1/4	1416 "	1850 "
2	a	25/32 x 1/4	1382 "	2028 "
	b	27/32 x 1/4	1434 "	2092 "
3	a	27/32 x 1/4	1743 "	2543 "
	b	25/32 x 1/4	*1439 "	2099 " *
4	a	25/32 x 19/32	1190 "	1681 "
	b	25/32 x 19/32	1175 "	1660 "
5	a	13/16 x 19/32	1358 "	1845 "
	b	13/16 x 13/16	*525 "	690 " *

* Indicates a defective brickette.

SHRINKAGE TEST.

No.		Area Green	Area Burned	Per Cent. Loss
1	a	1. Sq. In.	.793 sq.in	20.70
	b	1. "	.765 "	23.50
2	a	1. "	.688 "	31.65
	b	1. "	.685 "	31.45
3	a	1. "	.685 "	31.45
	b	1. "	.685 "	31.45
4	a	1. "	.708 "	29.20
	b	1. "	.708 "	29.20
5	a	1. "	.736 "	26.40
	b	1. "	.761 "	23.83

TESTS OF SERIES. (Continued.)

A-III

ELECTRICAL RESISTANCE TEST.

No.		Thickness	Puncturing Voltage	Remarks:
1	a	1/16 inch.	————	Broke in burning.
	b	———	————	
2	a	1/16 "	18,050	
	b	1/16 "	10,250	
3	a	1/16 "	19,200	
	b	1/16 "	20,750	
4	a	1/16 "	38,250	
	b	1/16 "	Not punct'd	Arced around at 49,500 Volts.
5	a	1/16 "	" "	" " at 49,500 "
	b	1/16 "	————	Defective.

ABRASION TEST
With 200 Pounds Flint Pebbles in Wet Ball-Mill

No.		Before Test	After Test	Per Cent. Loss
1	a	38.5 grs.	37.2 grs.	3.25
	b	50.2 "	49.0 "	2.49
2	a	42.0 "	40.0 "	4.76
	b	45.2 "	45.2 "	——
3	a	45.5 "	43.5 "	4.40
	b	42.0 "	42.0 "	——
4	a	58.0 "	52.2 "	1.41
	b	47.0 "	47.0 "	——
5	a	45.5 "	45.5 "	——
	b	50.0 "	50.0 "	——

a=3000 revolution test.
b=1000 " "

ABSORPTION TEST.

No.		Before Soaking	After Soaking	Per Cent. Absorpt'n
1	a	33.5 grs.	38.5 grs.	13.16
	b	44.5 "	50.2 "	12.92
2	a	40.5 "	42.0 "	3.70
	b	43.0 "	45.2 "	5.23
3	a	45.5 "	45.5 "	——
	b	42.0 "	42.0 "	——
4	a	53.0 "	53.0 "	——
	b	47.0 "	47.0 "	——
5	a	45.5 "	45.5 "	——
	b	50.0 "	50.0 "	——

TENSILE TEST.

No.		Area of Cross Section.	Load Necessary to Break	Tensile Strength of 1 Sq. Inch
1	a	11/32 x 27/32	380 lbs.	498 lbs.*
	b	11/32 x 11/16	1035 "	1260 "
2	a	11/32 x 11/16	698 "	1059 " *
	b	11/32 x 11/16	1200 "	1820 "
3	a	11/32 x 11/16	1288 "	1749 "
	b	11/32 x 11/16	1215 "	1624 "
4	a	11/32 x 11/16	1408 "	2130 "
	b	11/32 x 11/16	1724 "	2812 "
5	a	11/32 x 11/16	1985 "	2902 "
	b	11/32 x 11/16	1567 "	2292 "

*Indicates a defective brickette.

SHRINKAGE TEST.

No.		Area Green	Area Burned	Per Cent. Loss
1	a	1. Sq. In.	.764 sq.in	23.54
	b	1. "	.821 "	17.88
2	a	1. "	.659 "	34.08
	b	1. "	.659 "	34.08
3	a	1. "	.736 "	26.37
	b	1. "	.748 "	25.20
4	a	1. "	.659 "	34.08
	b	1. "	.660 "	33.99
5	a	1. "	.683 "	31.64
	b	1. "	.683 "	31.64

TESTS OF SERIES. (Continued)

B-I.

ELECTRICAL RESISTANCE TEST.

No.		Thickness	Puncturing Voltage	Remarks:
1	a	3/16 inch	Not punct'd	Arced around at 47,850 Volts.
	b	3/16 "	" "	" " " 47,850 "
2	a	3/16 "	" "	" " " 47,850 "
	b	3/16 "	" "	" " " 47,850 "
3	a	3/16 "	17,100	
	b	3/16 "	16,200	
4	a	3/16 "	18,000	
	b	— "	———	
5	a	3/16 "	26,550	
	b	3/16 "	27,450	

ABRASION TEST
With 200 Pounds Flint Pebbles in Wet Ball-Mill

No.		Before Test	After Test	Per Cent. Loss
1	a	45.5 grs.	45.0 grs.	1.10
	b	42.5 "	42.0 "	1.17
2	a	41.0 "	40.0 "	1.22
	b	50 5 "	50.0 "	0.99
3	a	42.7 "	42.0 "	1.75
	b	41.5 "	41.0 "	1.20
4	a	43.7 "	43.0 "	1.71
	b	41.5 "	40.5 "	2.16
5	a	42.7 "	40.5 "	5.26
	b	38.5 "	38.2 "	0.64

a=2000 revolution test.
b=1000 " "

ABSORPTION TEST.

No.		Before Soaking	After Soaking	Per Cent. Absorpt'n
1	a	47.7 grs.	47.7 grs.	———
	b	42.5 "	42.5 "	———
2	a	51.0 "	51.0 "	———
	b	50.5 "	50.5 "	———
3	a	44.0 "	44.0 "	———
	b	41.5 "	41.5 "	———
4	a	45.7 "	45.7 "	———
	b	41.5 "	41.5 "	———
5	a	46.0 "	46.0 "	———
	b	38.5 "	38.5 "	———

TENSILE TEST.

No.		Area of Cross Section	Load Neces'ary to Break	Tensile Strength of 1 Sq. Inch
1	a	11/16 x 11/16	1455 lbs.	1977 lbs.
	b	11/16 x 11/16	1339 "	1959 "
2	a	11/16 x 11/16	990 "	1345 "
	b	11/16 x 11/16	1514 "	2129 "
3	a	11/16 x 11/16	1293 "	1891 "
	b	11/16 x 11/16	1104 "	1500 "
4	a	11/16 x 11/16	1550 "	2449 "
	b	11/16 x 11/16	1457 "	2302 "
5	a	11/16 x 11/16	637 " *	944 " *
	b	11/16 x 11/16	1171 "	1850 "

* Indicates a defective brickette.

SHRINKAGE TEST.

No.		Area Green	Area Burned	Per Cent. Loss
1	a	1. Sq. In.	.736 sq.in	26.40
	b	1. "	.683 "	31.64
2	a	1. "	.736 "	26.40
	b	1. "	.711 "	28.90
3	a	1. "	.683 "	31.64
	b	1. "	.736 "	26.37
4	a	1. "	.632 "	36.72
	b	1. "	.632 "	36.72
5	a	1. "	.675 "	32.50
	b	1. "	.632 "	36.72

TESTS OF SERIES. (Continued)

B-II.

ELECTRICAL RESISTANCE TEST.

No.		Thickness	Puncturing Voltage	Remarks:
1	a	1/16 inch	16,650	
	b	1/16 "	18,450	
2	a	1/16 "	15,975	
	b	1/16 "	18,900	
3	a	1/16 "	15,300	
	b	1/16 "	16,650	
4	a	1/16 "	17,100	
	b	— "	—	Punctured at 34,500 Volts, 5/8 in. thick.
5	a	1/16 "	19,125	
	b	— "	—	Punctured at 47,250 Volts, 5/8 in. thick.

ABRASION TEST
With 200 Pounds Flint Pebbles in Wet Ball-Mill

No.		Before Test	After Test	Per Cent. Loss
1	a	43.7 grs.	40.0 grs.	8.57
	b	45.5 "	44.5 "	2.20
2	a	45.0 "	44.5 "	1.10
	b	45.7 "	45.5 "	0.55
3	a	42.0 "	41.0 "	2.38
	b	41.5 "	41.5 "	—
4	a	40.5 "	36.5 "	10.00
	b	44.5 "	43.5 "	2.24
5	a	46 0 "	41.0 "	10.87
	b	49.5 "	43.5 "	12.12

a = 2000 revolution test.
b = 1000 " "

ABSORPTION TEST.

No.		Before Soaking	After Soaking	Per Cent. Absorpt'n
1	a	42.0 grs.	47.2 grs.	12.5
	b	40.5 "	45.5 "	11.1
2	a	39.0 "	39.0 "	—
	b	45.7 "	45.7 "	—
3	a	52.2 "	52.2 "	—
	b	41.5 "	47.5 "	—
4	a	55.5 "	55.5 "	—
	b	44.5 "	44.5 "	—
5	a	50.0 "	50.0 "	—
	b	49.5 "	49.5 "	—

TENSILE TEST.

No.		Area of Cross Section	Load Necessary to Break	Tensile Strength of 1 Sq. Inch
1	a	15/16 x 1/4	623 lbs.	759 lbs.
	b	11/16 x 1/4	690 "	812 "
2	a	27/32 x 1/4	1352 "	1971 "
	b	27/32 x 1/4	1345 "	1962 "
3	a	15/16 x 1/4	1425 "	2004 "
	b	11/16 x 1/4	1980 "	2892 "
4	a	15/16 x 1/4	1565 "	2201 "
	b	11/16 x 1/4	1421 "	1925 "
5	a	15/16 x 1/4	1160 "	1631 "
	b	11/16 x 1/4	1173 "	1590 "

SHRINKAGE TEST.

No.		Area Green	Area Burned	Per Cent. Loss
1	a	1. Sq. In.	.820 sq.in	18.0
	b	1. "	.850 "	15.0
2	a	1. "	.685 "	31.4
	b	1. "	.685 "	31.4
3	a	1. "	.711 "	28.9
	b	1. "	.738 "	26.4
4	a	1. "	.711 "	28.9
	b	1. "	.738 "	26.4
5	a	1. "	.711 "	28.9
	b	1. "	.738 "	26.4

TESTS OF SERIES. (Continued.)

B-III

ELECTRICAL RESISTANCE TEST.

No.		Thickness	Puncturing Voltage	Remarks:
1	a	———	———	Burst in firing.
	b	———	———	
2	a	$\frac{1}{16}$ inch.	31,050	
	b	$\frac{1}{8}$ "	25,500	
3	a	$\frac{1}{16}$ "	Not punct'd	Arced around at 49,500 Volts.
	b	$\frac{3}{16}$ "	" "	" " at 49,500 "
4	a	$\frac{1}{16}$ "	" "	" " at 49,500 "
	b	— "	———	
5	a	$\frac{1}{16}$ "	Not punct'd	Arced around at 49,500 Volts.
	b	$\frac{3}{16}$ "	" "	" " at 49,500 "

ABRASION TEST
With 200 Pounds Flint Pebbles in Wet Ball-Mill

No.		Before Test	After Test	Per Cent. Loss
1	a	39.5 grs.	38.5 grs.	2.53
	b	45.5 "	45.0 "	1.10
2	a	44.0 "	43.0 "	2.27
	b	45.0 "	43.0 "	4.44
3	a	47.0 "	41.5 "	11.70
	b	57.5 "	57.0 "	0.87
4	a	49.5 "	48.0 "	3.08
	b	56.5 "	55.7 "	1.32
5	a	58.0 "	56.0 "	3.45
	b	51.5 "	51.0 "	0.97

a=2000 revolution test.
b=1000 " "

ABSORPTION TEST

No.		Before Soaking	After Soaking	Per Cent. Absorpt'n
1	a	42.0 grs.	42.0 grs.	——
	b	45.5 "	45.5 "	——
2	a	50.0 "	50.0 "	——
	b	45.0 "	45.0 "	——
3	a	50.7 "	50.7 "	——
	b	57.5 "	57.5 "	——
4	a	58.7 "	58.7 "	——
	b	56.5 "	56.5 "	——
5	a	56.0 "	56.0 "	——
	b	51.5 "	51.5 "	——

TENSILE TEST.

No.		Area of Cross Section	Load Neces'ary to break	Tensile Strength of 1 Sq. Inch
1	a	$\frac{14}{16}$ x $\frac{11}{16}$	825 lbs.	1118 lbs.*
	b	$\frac{14}{16}$ x $\frac{11}{16}$	2070 "	2805 "
2	a	$\frac{14}{16}$ x $\frac{11}{16}$	1220 "	1697 "
	b	$\frac{14}{16}$ x $\frac{11}{16}$	1511 "	2101 "
3	a	$\frac{14}{16}$ x $\frac{11}{16}$	1465 "	2223 "
	b	$\frac{14}{16}$ x $\frac{11}{16}$	1474 "	2247 "
4	a	$\frac{14}{16}$ x $\frac{11}{16}$	1755 "	2569 "
	b	$\frac{14}{16}$ x $\frac{11}{16}$	1715 "	2385 "
5	a	$\frac{14}{16}$ x $\frac{11}{16}$	1504 "	2194 "
	b	$\frac{14}{16}$ x $\frac{11}{16}$	1298 "	1805 "

*Indicates a defective brickette.

SHRINKAGE TEST.

No.		Area Green	Area Burned	Per Cent. Loss
1	a	1.0 Sq.In	.738sq.in	26.2
	b	1.0 "	.738 "	26.2
2	a	1.0 "	.719 "	28.1
	b	1.0 "	.719 "	28.1
3	a	1.0 "	.656 "	34.4
	b	1.0 "	.656 "	34.4
4	a	1.0 "	.683 "	31.6
	b	1.0 "	.719 "	28.1
5	a	1.0 "	.685 "	31.4
	b	1.0 "	.719 "	28.1

TESTS OF SERIES. (Continued.)

C—I

ELECTRICAL RESISTANCE TEST.

No.		Thickness	Puncturing Voltage	Remarks:
1	a	1/8 inch.	84,000	Arced around at 47,500 volts.
	b	1/4 "		
2	a	1/8 "	25,750	
	b	1/4 "	47,000	
3	a	1/8 "	17,250	
	b	1/4 "	18,550	
4	a	1/8 "	22,000	
	b	1/4 "	21,000	Defective.
5	a	1/8 "		
	b	1/4 "	14,900	

ABRASION TEST
With 200 Pounds Flint Pebbles in Wet Ball-Mill

No.		Before Test	After Test	Per Cent. Loss
1	a	36.0 grs.	36.0 grs.	.00
	b	39.5 "	39.5 "	.00
2	a	36.5 "	36.5 "	.00
	b	40.0 "	40.0 "	.00
3	a	41.5 "	41.5 "	.00
	b	41.5 "	41.5 "	.00
4	a	38.5 "	38.5 "	.00
	b	37.7 "	37.7 "	.00
5	a	33.7 "	33.7 "	.00
	b	33.7 "	33.7 "	.00

a = 2000 revolution test.
b = 1000 " "

ABSORPTION TEST.

No.		Before Soaking	After Soaking	Per Cent. Absorpt'n
1	a	36.0 grs.	36.0 grs.	.00
	b	39.5 "	39.5 "	.00
2	a	36.5 "	36.5 "	.00
	b	40.0 "	40.0 "	.00
3	a	41.5 "	41.5 "	.00
	b	41.5 "	41.5 "	.00
4	a	38.5 "	38.5 "	.00
	b	37.7 "	37.7 "	.00
5	a	33.7 "	33.7 "	.00
	b	33.7 "	33.7 "	.00

TENSILE TEST

No.		Area of Cross Section	Load Necessary to Break	Tensile Strength of 1 Sq. Inch
1	a	17/32 x 17/32	2094 lbs.	2942 lbs.
	b	17/32 x 17/32	1494 "	2099 "
2	a	17/32 x 17/32	1361 "	1985 "
	b	17/32 x 17/32	2105 "	3071 "
3	a	17/32 x 17/32	1585 "	2404 "
	b	17/32 x 17/32	1330 "	2018 " *
4	a	17/32 x 17/32	1686 "	2555 "
	b	17/32 x 17/32	1548 "	2308 "
5	a	17/32 x 17/32	1378 "	2168 "
	b	17/32 x 17/32	1729 "	2619 "

*Indicates a defective brickette.

SHRINKAGE TEST

No.		Area Green	Area Burned	Per Cent. Loss
1	a	1. Sq. In.	.7118sqin	28.82
	b	1. "	.7118 "	28.82
2	a	1. "	.6855 "	31.45
	b	1. "	.6855 "	31.45
3	a	1. "	.6592 "	34.08
	b	1. "	.6592 "	34.08
4	a	1. "	.6601 "	33.99
	b	1. "	.6601 "	33.99
5	a	1. "	.6848 "	36.52
	b	1. "	.6601 "	33.99

TESTS OF SERIES. (Continued)
C-II.

ELECTRICAL RESISTANCE TEST.

No.		Thickness	Puncturing Voltage	Remarks:
1	a	1/8 inch	———	
	b	1/4 "	13,500	
2	a	1/8 "	14,500	
	b	1/4 "	15,200	
3	a	1/8 "	14,000	
	b	1/4 "	14,300	
4	a	1/8 "	14,250	
	b	1/4 "	23,750	
5	a	1/8 "	18,750	
	b	1/4 "	36,000	

ABRASION TEST
With 200 Pounds Flint Pebbles in Wet Ball-Mill

No.		Before Test	After Test	Per Cent. Loss
1	a	30.0 grs.	30.2 grs.	8.33
	b	33.0 "	29.0 "	12.12
2	a	30.0 "	29.7 "	0.83
	b	30.7 "	30.2 "	1.63
3	a	33.0 "	33.0 "	0.00
	b	28.5 "	28.5 "	0.00
4	a	30.5 "	30.5 "	0.00
	b	30.5 "	30.5 "	0.00
5	a	32.0 "	31.2 "	2.34
	b	30.0 "	29.0 "	3.33

a = 2000 revolution test.
b = 1000 " "

ABSORPTION TEST.

No.		Before Soaking	After Soaking	Per Cent. Absorpt'n
1	a	29.0 grs.	33.0 grs.	13.8
	b	29.0 "	33.0 "	13.8
2	a	29.5 "	30.0 "	1.7
	b	30 0 "	30.7 "	2.5
3	a	33.0 "	33.0 "	———
	b	28.5 "	28.5 "	———
4	a	30.5 "	30.5 "	———
	b	30.5 "	30.5 "	———
5	a	32.0 "	32.0 "	———
	b	30.0 "	30.0 "	———

TENSILE TEST.

No.		Area of Cross Section	Load Necessary to Break	Tensile Strength of 1 Sq. Inch
1	a	7/8 x 7/8	848 lbs.	1101 lbs.
	b	31/32 x 29/32	876 "	1145 "
2	a	31/32 x 15/16	1018 "	1604 "
	b	31/32 x 15/16	1084 "	1706 "
3	a	15/16 x 15/16	410 "*	——— "*
	b	15/16 x 15/16	1157 "	1753 "
4	a	31/32 x 15/16	1140 "*	1796 "*
	b	31/32 x 15/16	1643 "	2585 "
5	a	15/16 x 15/16	705 "*	——— "*
	b	31/32 x 15/16	1492 "	2350 "

* Indicates a defective brickette.

SHRINKAGE TEST.

No.		Area Green	Area Burned	Per Cent. Loss
1	a	1. Sq. In.	.765 sq.in	23.44
	b	1. "	.764 "	23.56
2	a	1. "	.634 "	36.52
	b	1. "	.634 "	36.52
3	a	1. "	.660 "	33.99
	b	1. "	.660 "	33.99
4	a	1. "	.634 "	36.52
	b	1. "	.634 "	36.52
5	a	1. "	.634 "	36.52
	b	1. "	.634 "	36.52

GENERAL CONCLUSIONS ON THE WORK AS COMPLETED.

The object of this investigation was to prove, if possible, the proportions of the essential constituents which go to make up the best insulating porcelains. By the test, we have learned that porcelains of equally good appearance may possess greatly varying values when subjected to various tests. The essential properties of an electrical insulator are: That it possess, first of all, a high efficiency as a non-conductor. Second, that it stand, without snapping, any reasonable strain put upon it by the exigencies of wiring. Third, that it withstand the blows of missiles, even bullets, without breaking. The electrical, the tensile and the abrasion tests, respectively, were made to show these properties in the porcelains tested.

From a study of the electrical resistance tests we have found that every specimen standing a superior test has a high silica and moderately low alumina content. Unfortunately in each series showing the variation in proportions of CaO and K_2O, the content of Al_2O_3 was so high that no exceptionally good porcelains were obtained, even with high K_2O content. However, a study of tables A-II, B-II and C-II will show the reader that a marked improvement is apparent upon increasing the K_2O and decreasing the CaO, except at the extreme of the series where we see the K_2O used alone, and here the specimens show weakness due to brittleness. I infer from this study that had the K_2O been somewhat increased, and the CaO proportionately decreased in series I and III, these series would have proved much better.

From a study of the abrasion tests, it is very apparent that with K_2O alone as a flux, and even with a moderately high clay substance, we obtain a brittle and weak porcelain. When as low as 20 per cent of K_2O is replaced by CaO, we find the product changed to a tough but well vitrified body, although I believe that the translucency is reduced by any additions of CaO. To just what extent this is the case is not a matter of importance in this work. In the series showing the effect of Al_2O_3 content, I notice

no great difference in the ability of the specimens to resist abrasion. Much the same statement would apply to the series showing the results of varying of SiO_2. However, I note that the medium high SiO_2 specimens seem to chip worse than the extremely high SiO_2 specimens, while the low SiO_2 specimens show a lack of ability to resist abrasion.

A study of the test tables will show the reader that of the forty different porcelains tested, we find six to be more or less porous. It appears that the statements made in discussing the tensile tests hold here also, and that the same limits may be accepted. I also note that CaO when substituted for an equal equivalent of K_2O, does not at either cone 6, cone 9, or cone 12, produce an equal amount of vitrification.

My conclusions from a study of the tensile tests are that a moderately high SiO_2 and Al_2O_3 content will bring a good strong porcelain, but that not over 1. Al_2O_3 and 6.2 SiO_2 should be used. None of the specimens below .8 Al_2O_3 and 4.2 SiO_2 were safe, owing to their very narrow vitrification limits. The RO that proved most satisfactory in the tensile tests was between

$$\left\{\begin{matrix}40 \text{ per cent. } K_2O \\ 60 \text{ per cent. } CaO\end{matrix}\right. \text{ and } \left.\begin{matrix}80 \text{ per cent. } K_2O \\ 20 \text{ per cent. } CaO\end{matrix}\right\}$$

The shrinkage tests tend to show, I think, that there are three factors that may cause a variation: (1) The amount of clay substance introduced. (2) The variation in the kind of flux used. (3) The amount of silica present. Hence the shrinkage may be varied to suit almost any case.

In closing this paper, I think I may say that in my opinion an ideal porcelain for electrical purposes, may mature at any point between cone 6 or cone 12. It may contain from 50 per cent. to 80 per cent of its RO in the form of K_2O and the remainder as CaO. It may contain from .8 Al_2O_3 to 1.0 Al_2O_3 and from 4.2 SiO to 6.2 SiO_2. Expressing the same fact in formula form, we would have

$$\left.\begin{matrix}0.5 \text{ to } 0.8\ K_2O \\ 0.5 \text{ to } 0.2\ CaO\end{matrix}\right\}\ 0.8 \text{ to } 1.0\ Al_2O_3\ \left\{\ 4.2 \text{ to } 6.2\ SiO_2\right.$$

as the practical range of composition.

Closer limits than these I do not feel able to lay down, as the choice would be largely a matter of fancy for more translucency or more stoniness to be decided by the party interested. However, I think I may safely say that any porcelain within the limits named above, would, all other things being equal, be as good as, if not superior to the average electrical insulator porcelain now on the market.

DISCUSSION.

The Chair: Mr. Watts is certainly to be congratulated upon the able manner in which he has handled this subject. It is the first composition which has ever appeared in this or any other country on porcelain in which the matter of composition and electrical resistance are brought into comparison. I know you are all thinking now how hard it is going to be to discuss such an enormous mass of facts as Mr. Watts has patiently accumulated and presented here today. It certainly deserves some discussion, and an expression from the society of the recognition of its merits. I think Professor Orton would probably be glad to start the discussion. He has probably given some thought to the subject before he came here.

Professor Orton: I cannot say anything about the paper except to express my admiration for the patience and fidelity with which Mr. Watts has carried out his task. It has involved three months' patient labor, and there were a good many occasions upon which he was obliged to sit up all night, in order to carry through a test; and I think I may fairly say that there are not so very many people in this society or outside of it, who would be willing, for the sake of scientific advancement, to devote three months of their time, without recompense, to an investigation of this sort. In my opinion, it marks an era in the life of this society, when a young man of training, opportunity and experience, is willing to present a subject upon which so little is known and upon which so high a commercial valuation is placed.

The transactions of the electrical societies of the world contain records of a great variety of tests on electrical resistance, and the subject has been very carefully studied by

them to find a material to help out in the handling of high tension currents. But so far as I am aware (Mr. Watts will correct me if the statement is not true), no one has before attempted to show the relation between the composition of porcelain, and its ability to resist high tension currents; and I consider it a great honor for the American Ceramic Society to be the medium of presentation to the world of the first authentic information of this character. As far as discussing the paper on its merits is concerned, I am entirely unable to do so.

It occurs to me to ask Mr. Watts to describe to the society the way an electric current acts on arcing around an obstruction or insulation. Can you show us just how that occurs?

Mr. Watts: (Makes illustration on blackboard). Those of you who have had experience with any kind of electrical work know that when a current reaches a certain tension, the tendency is to complete the circuit, no matter what the distance. Electrical engineers have determined the actual voltage necessary to jump a certain distance of air; and as I stated in my paper, about five inches of air space will be arced through, or crossed by the current at a voltage of 55,000; and in order to puncture the insulator, you must have an insulating material extending beyond that distance or make the insulation less perfect. That is the reason so many of my tiles could not be punctured. I hoped that I had made them so thin that the resistance of the porcelain would not be equal to the pressure of fifty thousand volts, but I was mistaken.

The modern high-voltage electrical insulator is made with two or three bonnets or petticoats, surrounding a central hole in which the plug is attached, by which it is fastened to the pole. The electricity passing on the wire, in its effort to get to the pole, if it were not for these aprons, would have sufficient power to arc through the air to the pole, at say thirty thousand volts. But by the use of these large aprons, it must traverse a much longer distance. That is the feature of the bell or petticoat insulator. The arcing consists of the electricity being transported across an

open space. In this case, it will follow the surface of a non-conductor rather than arc through the air. It almost invariably does this, and that is the reason why the bell insulator has such greatly extended surfaces. Does that answer your question, Professor?

Professor Orton: Yes, I think so. I would like to ask what is the relation between these bodies you have been exploiting here, and the general types of bodies in use in the electrical porcelain business. Have electrical porcelain manufacturers developed anything like a type for electrical porcelain, so far, or is each working for himself and keeping quiet?

Mr. Watts: Any man having a porcelain insulator standing fifteen or sixteen thousand volts feels that he has a gold mine, and will not let any information concerning it get out. Manufacturers having a material standing over that voltage have taken the greatest pains to prevent the composition of the body being known. I know of several instances where they have gone to ridiculous extremes to keep the matter secret. The bodies of high resisting porcelains in use in the United States, so far as I am familiar with them, use potash almost exclusively as the flux. Some few may contain a small quantity of lime. The tests I have made strengthen my belief, that a potash-lime porcelain is better able to endure strains than pure potash porcelain. You must understand that insulators are required to carry an immense strain, because high tension currents are carried on very large heavy copper wires, and the blowing of the wind on this wire frequently breaks the head of a large insulator short off.

Professor Orton: Do you know whether a red burning clay of fine vitrifying qualities has ever been used for insulation?

Mr. Watts: I know of one instance where a buff-red clay has been used successfully. But as far as I know, it did not stand a test of more than about 80,000 volts, while the porcelain which I made for this same party stood a test of 120,000. I do not believe a red burning body would stand as severe a test as the white or true porcelain.

Professor Orton: Does the presence of iron in the porcelain form the weak spot? Would the electric current break through a ferrous or ferric silicate easier than through a potash silicate?

Mr. Watts: I cannot say. But I know that any tendency to small cavities in the porcelain weakens it remarkably in resisting strains. It must be thoroughly vitrified so that moisture cannot penetrate it. If the moisture penetrates it, it decreases its resistance very fast. Dr. Zimmer cited a case last year of a kaolin vitrifying at a very low temperature. I happened to have some of the same sort of clay myself, and I put a piece of the raw clay into the kiln, and fired it to cone 02 and it came out magnificently vitrified. I never saw a piece of porcelain break with a more conchoidal fracture, yet it was somewhat ferruginous.

Professor Orton: What I want to know is whether a body of that kind would have a high insulating character?

Mr. Watts: That I do not know. I do not believe from what I have seen that the presence of iron had any deteriorating effect on the body used. I know of a number of clays being used in electrical insulating bodies, which have quite noticeable iron specks in them; and while these are not segregated and are evenly distributed, at the same time there is no indication of a deterioration of the product, owing to their presence, unless there is a big bubble or cavity of some sort formed by them.

Mr. E. C. Stover: Year before last, when we were at the power plant at Niagara Falls, I got hold of the head electrician there and asked him some questions along this line, as regards the different make-up of porcelain insulators and their relative resistance to electrical current, and his reply was that it was a case of mechanical strength rather than a case of chemical constitution. He claimed that perfect glass would make a thoroughly good insulator, but it was not used, because it was not so strong as a good solid porcelain. And I think some of the tests given there show that point quite clearly, as regards mechanical strength.

THE USE OF LEAD ORES IN CERAMICS.

BY

H. A. WHEELER, E. M., ST. LOUIS, MO.

In the use of lead in raw glazes, fritts, or glasses, for obtaining low fusing points, or soft enamels, or a highly vitreous lustre, the usual compounds employed are the oxides, litharge (PbO) and minium or red lead (Pb_3O_4), or the carbo-hydrate, white lead ($2\,PbCO_3$, $Pb\,(HO)_2$), which are all artificial products. When obtained from reputable dealers they are usually reasonably pure, the impurities* seldom exceeding three per cent. and often less than one per cent.

They generally come fine enough for immediate use, without further grinding, and they can be used direct for either raw or fritted glazes. But they are liable to cause lead poisoning if there is a lack of cleanliness in their use, and they are expensive, costing from five to ten cents per pound, according to the quality, quantity and market.

The natural ores of lead frequently occur sufficiently pure that they can be successfully substituted for these manufactured compounds in most of the requirements of ceramics, while two them, the sulphate or anglesite and the sulphide or galena, are free from poisonous effects, which alone should cause them to be used wherever it is possible, in preference to the manufactured compounds. These ores, or natural compounds of lead, are:

Cerussite, or "lead dry-bone," or the carbonate of lead.

Anglesite, or "hard carbonates," or the sulphate of lead.

Galenite, or galena, or "mineral," or the sulphide of lead.

There are other lead minerals that can be found in museum collections, but they occur in too small quantities and are too scarce to interest the ceramist.

* The dry white lead, not ground in oil, is of course alluded to. It is far less apt to be adulterated with baryta, as often occurs in the oil-ground product.

The carbonate, or cerussite ($PbCO_3$), contains 83.5 per cent. PbO or oxide of lead (or the active fluxing constituent in all natural or artificial lead compounds) when pure, and it can be used raw or in fritts or fused glazes direct without further preparation than grinding. It is a soft white, grayish or brownish colored mineral, that has a non-metallic lustre, and is very friable or easily ground. It is found in very large quantities in many mines in the Far West, and, like the sulphate or anglesite, is found in the upper portions of the mine, but never in depth or below the water-level, where the lead always changes to the sulphide or galenite. Notable occurrences are found in Leadville and in Chaffee county, Colorado, at Bingham and Tintic, Utah, and at Eureka, Nevada. In the Mississippi Valley lead districts of Missouri, Iowa and Illinois, this carbonate ore is only found in small amounts, though occasionally in ample quantities to meet the requirements of the ceramist.

As found in nature, carbonate ore is usually so mixed with the brown oxide of iron (limonite) and sometimes clay and other silicates, as to be valueless in ceramics; but pockets of many tons occasionally occur of a white to light gray color, that carry less than 5 per cent. of impurities, and hence, could be substituted for the more expensive manufactured compounds in many cases. Like the artificial carbonate and oxides of lead, cerussite is poisonous if taken internally, or absorbed through the skin, if lead poisoning is possible by such means, which will be taken up later.

The sulphate or anglesite ($PbSO_4$) contains when pure 73.6 per cent of oxide of lead, and it can be used direct in raw or fused glazes after grinding. It is a soft white to dirty gray colored mineral that has a non-metallic or earthy lustre, is usually very compact and heavy, but grinds readily. It is found quite abundantly in the far West, often in the same mine with the carbonate; Aspen and Eagle River, Colorado, and Frisco, Utah, have produced large quantities. The Mississippi Valley lead districts only produce it in trifling amounts, usually as a dirty gray exterior or shell on the galena or sulphide.

The sulphate is less apt to be contaminated with iron and other impurities than the carbonate, and by careful hand picking one would frequently be able to sort out several tons in many of the mines, that would have less than 5 per cent. of impurities. The dirty, dark color that it frequently has would suggest gross impurities, but the latter will often be found to aggregate less than 3 per cent. in very unattractive looking material. In the writer's opinion, the sulphate of lead is not poisonous, which makes this salt a very desirable form to use, whether the natural or artificial sulphate is employed.

Galenite or galena or the sulphide of lead (PbS) contains 86.6 per cent. Pb of metallic lead when pure, or 95.3 per cent. PbO or oxide of lead when the sulphur (S) has been driven off in an oxidizing atmosphere such as is almost invariably used in ceramics. Before the lead oxide in galenite or the sulphide of lead is available, it is necessary to roast and drive off the sulphur with which it is combined, which is accomplished by heating it to a red heat with an excess of air. As this is the condition of about all kilns or muffles when being heated, the galena or sulphide can be used direct in raw glazes, as the sulphur will roast and be driven off by the time the glaze has melted. But in fritting or fusing in crucibles, where the air is more or less completely shut off, galena or the sulphide can not be used, as most of it will melt and settle to the bottom as a matte in a raw or uncombined condition, since the silicic, boracic or phosphoric acids that all glazes depend on to make them fusible, are not able to decompose the sulphide of lead, and, therefore, the sulphur is not expelled. Where it is possible to use such oxidants as potash or soda nitre in the charge, the sulphur is expelled or else converted into the harmless SO_2 condition, and this valuable lead salt can then be employed for fused glazes, as all the lead is then rendered available. *

* If nitre is used to drive off the sulphur in galena, care must be exercised to not heat the charge too rapidly, least the combustion of the sulphur causes such a rapid evolution of SO_2 or SO_3 gases as to result in the charge boiling over.

Galena or galenite is a bluish-gray, metallic, very heavy mineral that is very brittle and quite soft. It occurs very abundantly in most of the mining districts of the far West, where it is usually far more valuable for its silver than its lead, as it may contain from 20 to 500 ounces of silver per ton, which is true of all western lead ores, whether sulphide, sulphate or carbonate. In the Mississippi Valley it is free from silver, while it usually occurs purer than in the far West, and it is very abundant, from the ceramic standpoint, in southeastern, central and southwestern Missouri, northwestern Illinois, southeastern Wisconsin, and northeastern Iowa. Excepting in southeastern Missouri, the galena is usually associated more or less intimately with zinc and iron minerals, that would gravely interfere with its ceramic value.

In the southeastern Missouri district, of which St. Louis is the outlet, the lead occurs in two radically different forms or types of deposits, known as the "disseminated" and "surface" mineral. The "disseminated" type of galena deposits occurs notably in St. Francois county, where the lead is found as a very low grade ore finely scattered or disseminated in a magnesian limestone. This is crushed and concentrated by water in mills to a product that will assay from 60 to 75 per cent. lead, the practice varying at the different mills, so that there are still 12 to 20 per cent. of impurities, which is mainly fine magnesian limestone of quite uniform character. There is a product that comes from the setting boxes of the slime machines in the concentration mills that is in a very fine condition, and which in some of the mills will assay 75 to 78 per cent. of lead, and which would be the most attractive material for the ceramist from this district.

The "surface" type of galena is mainly found in Washington and Jefferson counties, and Palmer and Potosi are the principal producing points. This surface or "block-mineral" consists of large cubes and chunks of galena that are found in the ferruginous or red surface clays, and which after a limited amount of picking with a cleaning hammer, or washing with a hose or water-bucket, will assay 80 to 85

per cent. in lead, or contain only 3 to 8 per cent. of impurities. By specifying a *specially selected* or extra clean mineral free from clay, carload lots of this coarse, lump galena can be secured that will assay 84 to 85.5 per cent. lead, or contain only 1½ to 3 per cent. of impurities, or pure enough for almost every use of the ceramist. The usual prices of the market run of this "surface" galena vary from $40.00 to $50.00 per ton, while by paying one-half cent. a pound extra, the very pure extra clean mineral that assays about 85 per cent. lead can be secured at, say three cents per pound, which is the richest lead compound on the market, excepting the two oxides minium and red lead, and by far the cheapest.

The galena or sulphide of lead is not poisonous from absorption through the skin and *probably* is not, when taken internally. The writer has strong doubts whether the lead poisoning, so frequently found among painters, lead-glaze dippers, lead smelters, etc., is ever due to absorption through the skin. For while the hands of this class of workmen are frequently more or less covered with oxide or carbonate compounds of lead, which would permit of its ready absorption through the pores, an intimate acquaintance with lead workers, leads the writer to believe that the lead is introduced into the human system by the food and tobacco that is handled by these same lead-befouled hands. It is a most painful fact that familiarity causes such men to become very careless and indifferent to the evils of lead poisoning, so that a man will sit down and eat his lunch without taking the trouble to first wash his hands, although they may be grimy with lead. The constant handling of tobacco, also, without any effort to protect the surface from contamination is a prolific source of trouble. Even where the wash basin is freely used, the dirt under the finger nails is seldom carefully attended to, and while this source of poisoning the food does not operate so rapidly as the unwashed hand, it is always loaded, and slowly but surely gets in its disastrous cumulative work. It is especially this source of contamination that causes the writer to doubt the poisonous character of galenite when taken internally, since his experience with many hundreds of workers in the lead mines and

concentration mills has failed to show any poisonous results. Yet, as soon as this sulphide is converted more or less freely into carbonate and oxide in the roasting and smelting furnaces, lead poisoning becomes extremely common among the same class of men, although the lead miners are even more careless than the lead smelters about washing their hands before eating. It would therefore seem that the lead mine workers probably take considerable more lead into their system in the course of a year than the lead smelters; but as it is in the form of galenite or the sulphide, which is insoluble, and as the gastric juices seem to have no effect on it, it seems to pass through the system and is promptly thrown off without producing any poisonous effects, or accumulating in the manner which makes the oxide and carbonate forms of lead so dangerous.

The writer further believes that the sulphate of lead, whether natural or artificial, is as free from poisonous action as the sulphide, as it is a salt of even greater insolubility; but his personal experience is not so extensive with this salt of lead. The writer is well aware of many lead mines in the far West that are most disastrous on the health of the miners, from the different forms of lead poisoning that result from working in them. In one case in Utah, from four to six weeks was as long as a man could withstand it, before being returned to the hospital. While in some of these cases, the sulphate or anglesite ore occurred, the writer always found it freely associated with the cerussite or carbonate, and, in the very dangerous mines, it was always the carbonate form. As these oxidized forms of lead ores are only found in *dry* mines, the severely poisonous character of such mines is usually due to the lead dust that floats in the mine atmosphere, resulting from the blasting, especially as lead ore is a favorite tamping for the blast holes.

The writer warmly commends the sulphide or sulphate compounds of lead to the ceramist to avoid the evils of lead poisoning, and as the sulphate will be found preferable, if not essential, in all crucible work, the artificial sulphate should be used if a satisfactory quality of the natural cannot be procured.

DISCUSSION.

Mr. Hensel: We are aware that American factories are consuming vast amounts of raw lead glazes, and it seems to me it would be of interest to discuss this question. In Germany, dealers in ceramic materials advertise that they have lead ores of high grade for sale which are reliable. We have nothing of that kind in America, and in buying lead ore for ceramic purposes we have to take the chance of getting a good grade. That is a great drawback. It seems to me that if those owning lead mines and putting the ore on the market would take some pains to learn what we need, and to supply a strictly uniform article, they could easily build up a very satisfactory branch of trade which is now entirely overlooked.

Professor Orton: I do not know whether Professor Wheeler is prepared to prove his statements or not, as to the non-poisonous quality of galena. If it can be proven by a collection of medical testimony, or by any other means such as doctors employ to find out such facts, it would be a valuable thing to know. And further, if we should use the sulphide of lead, as Professor Wheeler suggests, as a glaze ingredient, we would only be going back to the original practice of the English potter. The poisonous character of lead glazed ware in the beginning was due to the fact that the potter supplied no silica or alumina with the lead, which took up from the body itself enough to more or less completely satisfy its needs. Now, you can readily appreciate the fact that where the potter dusted powdered galena on a freshly thrown piece with his hands, he would get it very uneven, thick in some places and thin elsewhere. Where the glaze was thick, it was basic, and where it was thin, it satisfied itself with silica and alumina from the body. So those old English crocks were sometimes safe, and sometimes they were not. In consequence, all lead-glazed crockery came into very ill odor in England. But the ease with which glazes can be made with lead has caused potters to resist all attempts to down it by legislation in England and elsewhere, and it is still used and likely to be used.

The question is, cannot this suggestion of Professor Wheeler find a proper reception among individuals using lead glazes for the cheaper line of products—architectural products, glazed brick, the lower grades of floor or wall tiles, such as are used in foreign countries on a large scale, roofing tiles, &c. Why could not these industries use galena, and obtain a glaze which would be good enough for their purposes? It is not necessary that a crude architectural product have a glaze as fine as dishware, which is to be scrutinized at close range.

Mr. Hensel: Galena gives us as good a glaze as any other form for such wares as roofing tile.

Professor Orton: I never saw it tried, bnt I am glad to hear Mr. Hensel say that. I suppose the only objection would be the sulphur, but if that were properly roasted out, there is no reason why the lead oxide remaining is not as good as if introduced from any other source.

TIN OXIDE IN CERAMIC FLUXES.

BY

STANLEY G. BURT, CINCINNATI, O.

The main object of this paper is to determine whether any stannic oxide is soluble in a glaze and not to discuss tin enamels in general, as might be supposed. It is customary among potters to consider tin oxide as wholly insoluble in a glaze. The glaze in its fused state is thought to be unable to dissolve the tin at all, the particles merely floating in it, or as we say, being held in suspension.

It may possibly not be strictly correct to speak of a glaze forming a solution, since we form solutions of solids or gases by dissolving them in a liquid, so a glaze which when cooled is a solid, could not form a solution. However, right or wrong, it is the constant practice to speak of a substance as soluble or not in a glaze, by which we mean that on adding this substance to our glaze, we either have a clear transparent glaze or else we have the substance undissolved, held in suspension, and an opaque glaze or enamel resulting.

This being understood, it will be plain that our ability to make a tin enamel must rest upon its insolubility in the glaze flux. Now the points to be considered in this paper are, *first*, is it completely insoluble, and *second*, is it insoluble as the dioxide, or does it undergo some chemical change?

In these days we can no longer speak freely of substances as insoluble or infusible. Flint and carbon are now fused, and German chemists make elaborate analyses of the amount of glass dissolved by water. So it seemed quite possible that a certain percent of tin oxide might be soluble in a glaze.

If we turn to the subject of tin in our chemistries, we find that stannic oxide is infusible and insoluble in acids and alkalies, but when fused with sodium hydrate it yields a

stannate, soluble in water. Tin is one of the intermediate elements that act as acids toward strong bases, and as bases to strong acids, so we have sodium stannate, and stannic chloride. Careful research through such works as were at my command, failed to throw any light on the subject of this paper.

It is quite obvious that the first thing to investigate is whether this tin oxide was absolutely inert in ceramic fluxes. That is, whether some chemical change takes place in the oxide when fused in a ceramic flux, even though it be insoluble in its changed form.

A word in explanation of what I mean by a ceramic flux. In these, I include glazes and their low fusing ingredients considered separately, and exclude such fluxes as are not ordinarily used in ceramics. We know, for instance, that the oxide of tin fused with sodium hydroxide yields a soluble salt, but sodium hydrate is not used by potters, and hence is not a ceramic flux.

Before testing the oxide in actual glazes, I made a few tests, trying first boracic acid calcined and uncalcined. As there was no alkali in this, there was no chance of forming a stannate, and so far as my knowledge goes, we have yet to form a tin borate. Still, I was anxious to see whether there could be any action at all, and was rewarded by an interesting result. I took first the flaky boracic acid, which in heating, melts in its water of crystalization, boils and gradually loses this water, requiring a high temperature to do so completely, and finally leaving as a result, a fine clear glass. The formula for this acid is B_2O_3, 3 H_2O. With the water driven off in calcining, we have left the B_2O_3. Now, to 1. Eq. of the flaky acid, I added 0.004 equivalents SnO_2. The fusion takes place as before, but the result is a fine white enamel. The interesting point is that I believe the stannic oxide has undergone a chemical change, for while the regular oxide is so completely insoluble, this enamel that I got was soluble in HCl, from which solution the Sn was precipitated as the orange sulphide with sulphuretted hydrogen. We had B_2O_3, 3 H_2O and added SnO_2. Is it possible that stannic acid (H_2SnO_3) is formed? It is hardly

possible that the acid could exist at a high heat without dissociation. The water of crystallization is, however, a necessary part of the reaction, for on testing, the calcined boracic acid was unable to show any action on the oxide at all.

In repeating my experiments, to test my results, I found some difficulty in reproducing this reaction with the flaky acid, and as my time was too limited to determine the cause of error, I would not give this test here at all, except for the possible light it may throw on the different appearance of the two enamels. That is, on contrasting the enamel formed from the calcined acid with that from the non-calcined, we find that in the former the actual particles can be noticed in suspension, while in the latter they are altogether too fine to be distinguished. It has, in fact, quite the look of a precipitate.

Borax was next tried. Here we introduce sodium into our equation. The borax on fusing acts much the same as the boric acid; that is, it melts, gives off its water of crystallization, the result being finally the well known clear bright glass. On adding the stannic oxide to it, however, the action is entirely different. In the first place, I was unable to produce the voluminous white enamel produced with the flaky acid, but on the other hand I was here able to produce a perfectly clear solution of the tin in the borax. We have in borax $Na_2O, 2(B_2O_3), 10\ H_2O$. To this we add SnO_2. I would like to have shown the same effect from the water of crystallization as shown with the acid, but there is less actual boiling, if I may so call it, and the swelling of the borax gives less chance for action on the tin in that state. What we do form here seems to be the sodium stannate, $Na_2\ SnO_3 + 2\ (B_2O_3) + 10\ H_2O$, which gives a clear solution. Naturally, if too much tin is taken, the flux will be unable to dissolve it all, and the excess will be merely held in suspension, and an enamel will result. I was able to dissolve 0.0129 Eq. SnO_2 in 1. Eq. of Borax. This is of course far below what the formula calls for, but the action is very slow, and a prolonged fusion would be necessary to determine just how much could be dissolved.

Calcined borax acted equally well, showing that the water of crystallization is not a factor in this reaction. I feel justified in assuming the formation of Na_2SnO_3 here, for if there was any chance of a tin borate being formed, that would occur with the boracic acid.

Here we have a clear case of the complete solution of a certain per cent of the stannic di-oxide in a ceramic flux. That is, in this flux, we have converted it to a stannate which was soluble.

Those of us familiar with color-making know that in certain receipts for the tin-chromium-pink color, borax plays a leading part; that is, its action on the tin enables us to form the color. I have here a sample of borax and tin, fired together without any chromium, in which you see the distinct formation of a lilac color.

We know that by fusing the tin oxide with NaOH, we have a ready means of forming the sodium stannate. While, as before stated, NaOH is not a ceramic flux, Na_2CO_3 is, so it seemed of interest to determine what its action would be. When fused alone, Na_2CO_3 will not give a clear glass on cooling, so it is impossible to see in what form the tin fused in it is present. The mass resulting from this fusion is only partially soluble in water. In this solution I could find no tin, but the total mass was soluble in HCl. In just what form the tin is present here, is hard to say. Its not being soluble in water would indicate that it was not the stannate. What is plain, however, is that we have here another ceramic flux which acts directly on the tin oxide.

As a final preliminary test, I added SiO_2 to the Na_2CO_3 until all effervescence ceased, forming thereby, sodium silicate. Now, to this I added tin oxide, aiming to determine thereby whether this silicate would have any action on the tin. I found that the action there was practically the same as with the carbonate — partial solution in water, complete solution in HCl. The tin easily precipitated as orange sulphide with sulphuretted hydrogen. This is a most important test. All glazes are silicates. Therefore, if in a silicate there was no direct action on the oxide, this

investigation would necessarily be much more complicated. We would very possibly be ruled out of court as having no case.

We would have to show that the tin went into an early combination, and in that way was soluble in the silicate flux. Failing in this, a careful investigation of various silicates would be necessary, a long and difficult problem. Fortunately, this is not necessary, as this test shows that the oxide of tin is not an inert substance in a silicate flux, and further we have shown that in the presence of borax, it may go into a complete solution. My next tests show that even without borax, we may get a complete solution.

I now tested two regular glazes with the stannic oxide. The first, a fritted glaze, having 1. equivalent B_2O_3, the second, a raw lead glaze, having no B_2O_3. To these I added the SnO_2, starting with 0.01 equivalent and running up to 0.25 equivalent. I have samples of these with me, and would be glad to have the members examine them. In the raw lead glaze, the 0.01 equivalent seems to be completely dissolved, while with 0.02 we can easily see a trace remaining. The 0.25 equivalent gives a complete enamel. In the fritted glaze not only is the 0.01 completely dissolved, but as far as I can determine, 0.02 has also gone into solution. It is therefore probable that the average glazes dissolve at least 0.01 equivalent of tin oxide.

The enamels formed by these two glazes are quite different. While the fritted glaze forms a fair enamel the raw glaze has a curdled look, and the small particles of tin are easy distinguishable. One of the points I hoped to prove in this paper, was my belief that certainly some of the tin was first dissolved and then precipitated in a true enamel. In speaking of the enamels from the calcined boracic acid, and that from the un-calcined, I called attention to the fact that while the former showed the particles in suspension, the latter had the true milky character. Much the same effect is noticed in the enamels from these two glazes. However, this is merely theory, for which as yet I have been unable to gather sufficient proof, so I will not dwell upon it further.

It may be asked what the direct practical value of such an investigation is. This question of the solubility of tin oxide was raised in a discussion of crystalline glazes. We know that certain elements combine with silica to form amorphous silicates, which give a clear glass or glaze. Now this combination takes place at a high heat. Hot liquids, as a rule, are able to hold in solution a greater per cent. of salt than cold ones, so that we often have a clear hot solution from which salts crystallize out on cooling. The same seems to be true of a glaze. While at its maximum heat, a glaze may be clear, even if supersaturated with certain elements. On slow cooling these may crystallize out, forming at times very beautiful results. We have been able to produce this effect from a number of elements, as for instance, zinc, iron, copper, chromium, titanium, but others remain to be investigated. If we are ever to attempt any such result from tin, we must of course first show that the silicate flux has a solvent action on the tin oxide, second, a proper supersaturated solution must be formed, and third, this must be tested with the careful cooling required by other crystalline glazes.

I feel that the first step has been accomplished, as it has been shown that a certain per cent. of tin is soluble in a glaze. The formation of a proper super-saturated solution should next be tried. As the sodium stannate is shown soluble in borax, it is possible that a super-saturated solution of this, added to a fritt under proper conditions, might lead to satisfactory results.

DISCUSSION.

Mr. Stover: (Placing on the blackboard the following formula:)

$$\left.\begin{matrix}.60\ CaO \\ .36\ K_2O \\ .04\ SnO_2\end{matrix}\right\} 0.5\ Al_2O_3,\ 4.00\ SiO_2 \qquad \text{(At Cone 12)}$$

There is a formula of a tin glaze which matures properly, making a good opaque glaze, at cone 12. The tin oxide in this glaze figures out about 1.5 per cent.

Mr. J. W. Hasburg: A paper involving such questions in chemistry is not very easy to discuss off-hand, but I will state some observations made by me a long time ago on tin glazes.

As stated by Mr. Burt, sodium and tin oxides in proper proportions form sodium stannate, a definite chemical compound soluble in water. Boracic acid and tin oxide in any proportion does not form a chemical compound, as all the boracic acid can be dissolved out, leaving the tin oxide unchanged, which shows that no chemical union has taken place. If, however, an alkali is added to this boracic acid-tin mixture, the tin will be dissolved in proportion to the alkali added.

Tin oxide is the most difficult oxide to dissolve in a silicate, and when more tin oxide is present than the alkali will take up, the mass becomes opaque; sometimes it comes from the fritt fire almost clear, and only becomes opaque after reheating.

On a close examination, it will be found to have a crystalline structure. If other bases having a greater affinity for alkaline silicate, or which dissolve more readily in it, are present, it will not require so much tin oxide to make this silicate opaque.

Titanic oxide acts very much the same as tin oxide, and other bases will also make opaque mixtures if added in excess, but they may require annealing to bring out the opacity and cause crystallization.

The opacity of tin glazes is due entirely, in my opinion, to the tin being in suspension.

The amount of tin oxide in the above formula is small on account of the alkaline silicates being saturated with other bases, which combine with the silicates more readily than tin oxide.

Mr. Hensel: Schumacher states that a lilac color like that which Mr. Burt shows is very often due to a combination of lime, tin and iron. A very small quantity of iron is all that is needed to develop the color.

NOTE ON THE RELATIVE POINT OF DEHYDRATION OF PURE AND CALCAREOUS CLAY.

BY

WILLIAM M. KENNEDY, BEAVER FALLS, PA.

The first experiment on this subject appearing in an American publication, was one performed by Mr. A. E. Barnes. It appeared in the "Clay Worker" for June, 1895. The incompleteness of Mr. Barnes' work was due to his having used a Brown pyrometer of the expansion bar type, the temperature limit of which is about twelve hundred degrees Fahrenheit. The dehydration period is generally construed to include, not only the driving off of the combined water from pure clay, but also the expulsion of CO_2SO_2, etc., from impure clays, such as calcareous or limey clays, ferrugineous clays and the like.

In this experiment I have made a comparison of two of these clays, namely, pure clay and calcareous clay.

I prepared two series of brickettes. The first series was made by grinding in a ball mill 3000 grams of "Florida kaolin," running this slip through a 150 mesh sieve and drying in plaster moulds. The clay was then thoroughly wedged and kneaded and sixty brickettes were made. The brickettes were moulded in the form of elliptical discs with rounded edges, about three-eighth of an inch in thickness and weighing in the neighborhood of thirty grams each. A one-fourth inch hole for inserting wire for removal from the kiln was made in each brickette, and each was carefully stamped with a number for identification. When thoroughly dry, the brickettes were carefully sponged, and polished with a cloth, to prevent loss in weight by chipping of corners and edges in handling.

The other batch of material had the following composition:

Florida Kaolin	70 parts.
Whiting	80 "
	100 parts.

This material was mechanically treated, moulded into brickettes, dried, marked and polished in the same manner as the first batch.

The dried brickettes were heated for one hour in an air oven at 130 degrees C. They were then cooled in a dessicator, and rapidly weighed to prevent absorption of hygroscopic water. The accuracy of weighing was to one milligram. The brickettes were then placed in a muffle in a down draft kiln. A fire-clay tube, three inches in diameter, containing the porcelain tube and thermo-couple of the Le-Chatelier pyrometer was laid lengthwise through the centre of the muffle, and the kaolin brickettes were now piled up on one side of the tube, and the kaolin-lime brickettes on the other side. The distance of the brickettes from the outside of the muffle was about twenty-four inches, and was in the centre of the heated area and well protected from drafts of cold air. The front of the muffle was left open, and opposite it, two large peep holes were left in the wicket, through which it was easy to reach in with a wire and remove the brickettes.

The brickettes being placed in position, the kiln was started. The temperature was carefully noted at every 50 degrees from 100 degrees to 300 degrees, and two of each kind of brickettes were withdrawn at each temperature reading, cooled at a dessicator and weighed. From 300 degrees to 850 degrees brickettes were withdrawn at every 25 degrees. From 850 degrees to 1000 degrees, the completion of the burn, brickettes were removed at every 50 degrees.

The time occupied by the experiment was twenty-eight hours. The temperature was increased as steadily and regularly as possible until 725 degrees was reached, when this heat was maintained as near as possible for a trifle over twelve hours, to observe the effect of heat-soaking on the brickettes. The temperature was then raised rapidly to 1000 degrees.

During the early stages of the burn difficulty was experienced in seeing the brickettes in the kiln, and a bicycle lamp was used to project light into the dark muffle. From 575 degrees up to the completion of the burn, no trouble was

experienced from this cause. In the early stages of the burn, the brickettes were sooty; this disappeared at 475 degrees in the calcareous clay brickettes, and at 500 degrees in the kaolin brickettes. When weighed after removal from the kiln, this soot was removed to as great an extent as possible with a fine "camel hair" brush. At 525 degrees the clay-lime brickettes showed a faint red glow. The kaolin brickettes did not show evidence of becoming "red hot" until 550 degrees was reached.

ANALYSIS OF THE RESULTS.

Taking the kaolin brickettes first, it will be observed (Plate I, black line,) at 150 degrees, the temperature at which the first observation was made, that only .018 per cent. of loss had occurred. From here to 450 degrees the loss was gradual, only amounting to 1.59 per cent. At this point, the brickettes began to lose weight rapidly. This rapid loss continued until a temperature of 600 degrees was reached, when 12.1 per cent. loss had occurred. From 600 degrees to 725 degrees the loss was again very gradual, the temperature was held near 725 degrees for about twelve hours in order to observe the effect of soaking on the expulsion of water. This, as it will be observed, was very slight, only .07 per cent. of loss occurring from this temperature to the end of the burn. Small losses occurred at each observation, aggregating in all only .54 of one per cent. The total loss was 13.84 per cent. and the dehydration period, or the temperature between which the bulk of the combined water, was driven off can with safety be said to lie between 450 and 600 degrees centigrade.

Now taking up the kaolin-lime brickettes (Plate I, broken line) the losses will be observed to be very slight up to 450 degrees. At 475 degrees but very little additional water had been expelled from these brickettes. From here to 600 degrees the rate of loss coincides almost exactly with that of the kaolin brickettes. As 600 degrees represents the close of the dehydration period, the losses from here on are due to the expulsion of CO_2 gas. The dotted line extending from 8.97 percent at 600 degrees to 9.7 percent at 1000

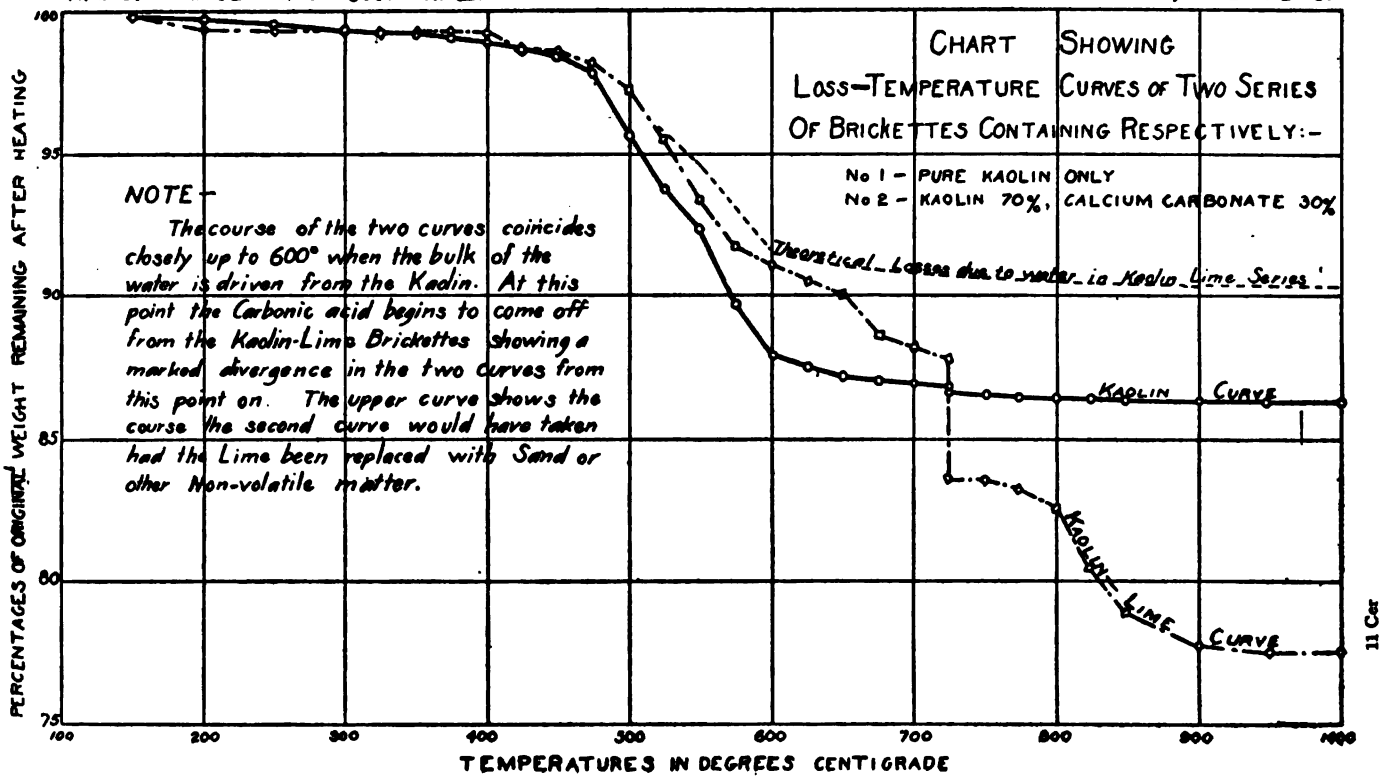


CHART SHOWING
LOSS-TEMPERATURE CURVES OF TWO SERIES
OF BRICKETTES CONTAINING RESPECTIVELY:-
No 1 - PURE KAOLIN ONLY
No 2 - KAOLIN 70%, CALCIUM CARBONATE 30%
NOTE -
The course of the two curves coincides closely up to 600° when the bulk of the water is driven from the Kaolin. At this point the Carbonic acid begins to come off from the Kaolin-Lime Brickettes showing a marked divergence in the two curves from this point on. The upper curve shows the course the second curve would have taken had the Lime been replaced with Sand or other Non-volatile matter.
Theoretical Losses due to water in Kaolin-Lime Series
KAOLIN CURVE
KAOLIN-LIME CURVE
PERCENTAGES OF ORIGINAL WEIGHT REMAINING AFTER HEATING
100
95
90
85
80
75
100
200
300
400
500
600
700
800
900
1000
TEMPERATURES IN DEGREES CENTIGRADE

degrees represents the theoretical loss of the remainder of water in the kaolin-lime brickettes. From 600 degrees to 725 degrees, the expulsion of CO_2 was almost as rapid as that of the water had been. 12.2 percent loss had occurred up to this point. The soaking period of 12 hours at this temperature shows a decided loss in the kaolin-lime brickettes—4.1 percent out of a total of 22.5 percent; but for the next 125 degrees raise in temperature very little loss occurred. From here to 900 degrees the loss was again rapid, 22.3 percent having occurred at this temperature. From here on to the end of the burn (1000 degrees) only 0.2 percent loss was found. This series of brickettes proves that while the water of dehydration in kaolin is expelled between 450 and 600 degrees, the CO_2 in brickettes containing 30 percent lime will prolong the dehydration period to 900 degrees. It also shows that heat soaking for a comparatively long period is of no advantage, because while it does drive off some of the CO_2, it does not drive off all of it, and the end of the dehydration occurs at the same temperature as if no soaking had been given.

Plate II was drawn for the purpose of showing what took place during the soaking period at 725 degrees.

The solid black line shows the time-loss curve of the kaolin brickettes. It will be observed that the bulk of the water was driven off in less than two hours, or between five and seven hours after lighting the kiln; and it will also be observed that the soaking period had very little effect on the brickettes. From 8 hours and 55 minutes to 28 hours the curve line is almost straight with a total loss of 1.74 percent during the period.

With the kaolin-lime brickettes it is quite different. It will be observed (broken line) that the loss continued at a fairly rapid rate during the whole of the burn, and that during the soaking period, 11 hours and 30 minutes to 24 hours, there was a loss in weight of 4.1 percent.

The following tables show my observations as recorded:

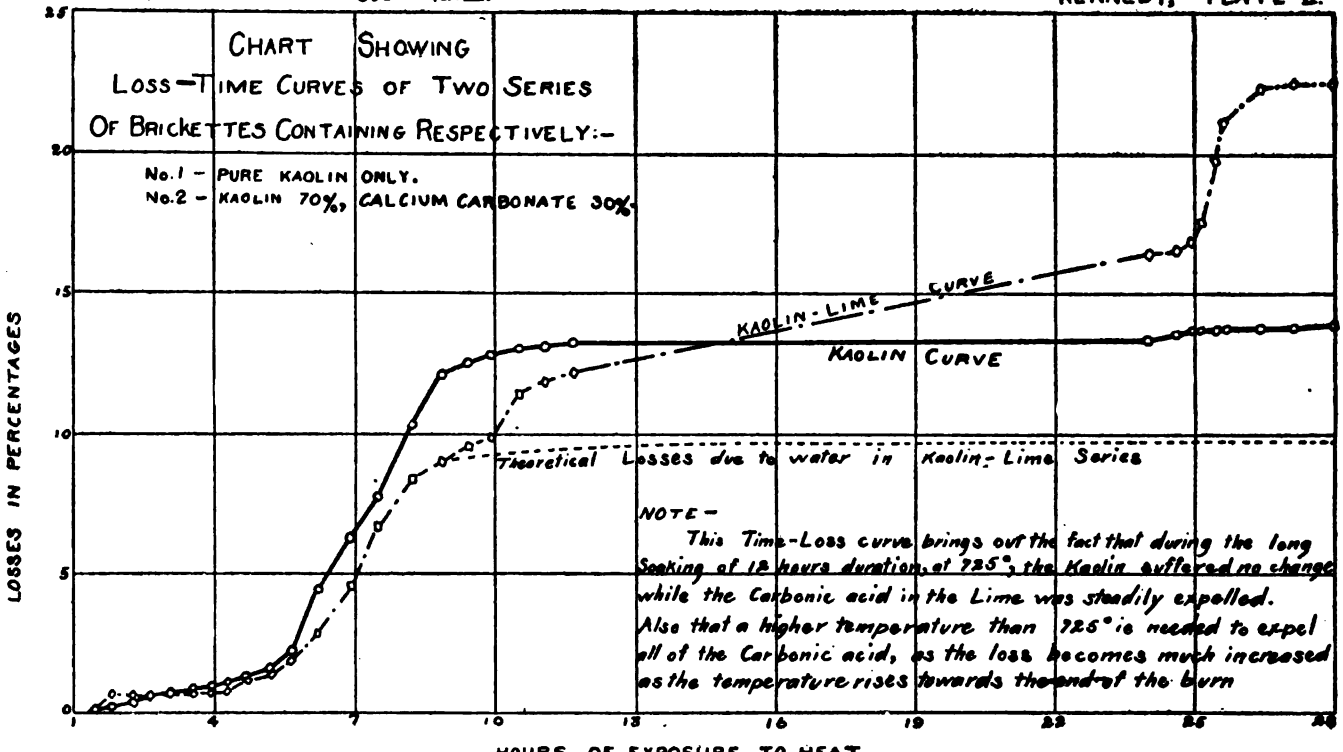


CHART SHOWING
LOSS—TIME CURVES OF TWO SERIES
OF BRICKETTES CONTAINING RESPECTIVELY:—
No.1 – PURE KAOLIN ONLY.
No.2 – KAOLIN 70%, CALCIUM CARBONATE 30%.
KAOLIN-LIME CURVE
KAOLIN CURVE
Theoretical Losses due to water in Kaolin-Lime Series
NOTE—
This Time-Loss curve brings out the fact that during the long Soaking of 12 hours duration, at 725°, the Kaolin suffered no change while the Carbonic acid in the Lime was steadily expelled. Also that a higher temperature than 725° is needed to expel all of the Carbonic acid, as the loss becomes much increased as the temperature rises towards the end of the burn
LOSSES IN PERCENTAGES
0
5
10
15
20
25
1
4
7
10
13
16
19
22
25
28
HOURS OF EXPOSURE TO HEAT

TABLE I.

PERCENT LOSS VS. TEMPERATURE.

Percentages of Loss in Kaolin Brickettes.				Percentages of Loss in Kaolin-Lime Brickettes.			
Temp. at Drawing	First Brickette.	Second Brickette.	Average.	Temp. at Drawing	First Brickette.	Second Brickette.	Average.
150	.006	.027	.018	150	.27	.075	.18
200	Chipped	.026	.026	200	.66	665	.66
250	.53	.29	.41	250	.58	.74	.66
300	.69	Chipped	.69	300	.67	1.22	.67
325	.73	.72	.72	325	.45	1.95	.71
350	.75	.75	.75	350	.55	.86	.71
375	.93	.95	.94	375	.77	.66	.71
400	Chipped	1.11	1.11	400	.44	1.13	.77
425	1.43	1.21	1.32	425	1.49	1.28	1.38
450	1.10	2.09	1.59	450	1.68	.985	1.41
475	2.41	2.10	2.25	475	Bad	1.84	1.84
500	4.45	Chipped	4.45	500	3.07	2.51	2.79
525	6.60	6.00	6 30	525	4.53	4.54	4.54
550	Bad	7.75	7.75	550	6.85	6.51	6.68
575	10.30	Chipped	10.30	575	8.20	8.55	8.35
600	12.10	12.10	12.10	600	8.54	9.40	8.97
625	12.60	12.45	12.52	625	9.53	Chipped	9.53
650	12.90	12.74	12.82	650	Chipped	9.90	9.90
675	12.90	13.10	13.00	675	11.40	Chipped	11.40
700	13.10	Chipped	13.10	700	Chipped	11.82	11.82
725	13.25	13.21	13.23	725	12.20	12.20	12.20
725	13.50	13.20	13.30	725	16.40	16.30	16.40
750	13.31	13.70	13.51	750	16.50	Chipped	16.50
775	13.58	13.65	13.61	775	17.10	16.40	16.80
800	Bad	13.65	13.65	800	17.30	17.60	17.45
825	Broken	13.67	13.67	825	20.00	19.52	19.76
850	13.70	13.60	13.70	850	21.20	21.00	21.10
900	Chipped	13.75	13.75	900	22.20	22.40	22.30
950	13.75	Chipped	13.75	950	22.50	22.50	22.50
1000	13.84	Bad	13.84	1000	22.40	22.54	22.50

TABLE No. II.

PERCENT LOSS VS. TIME.

Kaolin Brickettes.	Kaolin-Lime Brickettes.	Hours.	Minutes.
.018	.18	1	25
.026	.66	1	45
.46	.66	2	18
.69	.67	2	40
.72	.71	3	00
.75	.71	3	25
.94	.71	3	50
1.11	.77	4	15
1.32	1.38	4	45
1.59	1.41	5	15
2.55	1.65	5	40
4.45	2.79	6	15
6.30	4.54	6	50
7.75	6.68	7	35
10.30	8.35	8	15
12.10	8.97	8	55
12.52	9.53	9	25
12.82	9.90	9	55
13.00	11.40	10	30
13.10	11.82	11	10
13.28	12.20	11	50

Soaking period of 12 hours duration, during which no gain of temperature occurred.

13.30	16.40	24	00
13.51	16.50	24	40
13.61	16.80	24	55
13.65	17.45	25	10
13.67	19.76	25	25
13.70	21.10	25	40
13.75	22.30	26	10
13.75	22.50	27	20
13.84	22.50	28	00

DISCUSSION.

Mr. W. P. Gates: I have had some experiences in this line myself in our kilns. I used a natural limey clay, and I started drawing my trials much earlier. All were dried at 120° C. and placed in the kilns. On drawing up to about 150° C., the trials showed a decided gain in moisture, the highest amounting to four or five per cent. This gives a pretty good indication that a good deal of fuel could be lost from overdrying the wares. I think many plants dry their ware too much, whereas they could save by underdrying, and letting some of the moisture be taken off in the kiln, as the test showed that it had to be done over again in the kiln in any case.

Mr. Alfred Yates: My experience teaches me that you cannot get the ware dry enough; the dryer you set it, the better results you get.

The Chair: We all know that there is a large amount of water vapor in the kiln in the early stages of burning, and it would seem reasonable that an over-dried clay would re-absorb some of it.

Mr. Yates: I think that good burning requires a low temperature and good circulation in the kiln in the beginning. By this, you get even distribution. If you push it, you will have hot places and cold places, and the hot places will rob the cold places, and prevent equalizing over the whole area. But, if you will hold down the temperature, and keep up the circulation, you will get uniformity from the start.

Mr. W. P. Gates: I was speaking only of muffle kilns. This test of mine was run in a small muffle kiln. I do not know how it would run in an open kiln.

The Chair: The point in the paper is the comparison between clay with lime, and clay without it. Most of us have to deal with lime-bearing clays sometimes, and their behavior is decidedly interesting.

I would like to ask about the condition of the kaolin-lime brickettes at the close of the burn. Was there any difference in hardness?

Professor Orton: The lime-brick or the calcareous mixture was much more porous, of course, at the end of the burn than the kaolin was, because vitrification involving so large an amount as thirty percent of lime would not occur at so low a temperature; so the lime mixture was a good deal softer. If the temperature had gone higher, it would have been harder than the kaolin. So far as I know, the presentation of these facts here is the first time in which a pure clay has been compared with a calcareous clay in the matter of its dehydration curve; and it shows us a very interesting fact, viz., that the calcareous clay is ready for vitrification changes, and for the perfection of its heat reaction at 900°; while the clay-substance is practically ready from six hundred on. There is a stage in the dehydration period in which the gases are coming off the clay rapidly, which can be prolonged three hundred degrees by the presence of carbonate of lime. Two students in the university are now at work on a similar curve, in which ferrous carbonate is being experimented on in connection with kaolinite; the idea being to attempt to prove the point at which CO_2 is being expelled from $FeCO_3$. That is a more practical experiment, because clays containing thirty percent of lime are not commonly used. But clays containing ferrous oxide are in common use, and are especially subject to "black-coring," swelling and numerous other defects. Therefore it seemed to me quite important to show at what point a ferrous carbonate brick was ready for further combination, and so I assigned this problem to these students. This investigation of Mr. Kennedy's seems to me of equal value in a way, though not so directly applicable to conditions in this country.

THE MANUFACTURE OF ARTIFICIAL SAND STONE.

BY

SAMUEL V. PEPPEL, B. Sc., COLUMBUS, O.

During the last five or ten years, occasional newspaper articles written in popular vein have appeared describing a wonderful industry which was to revolutionize the brick business in this country, intimating that brick to answer all purposes were to be made from sand at a cost far below that of ordinary clay brick. Time has not verified these statements, in this country at any rate. Nevertheless, the sand brick industry has a promising future in this country, for the production of ornamental architectural effects, if for no other purpose.

Sand brick, or the "kalksandstein" of the Germans, is a mass of sand particles bound together by either calcium carbonate or calcium hydro-silicate, or mixture of both. Sand brick, with calcium carbonate as the cementing material, or "filler" of the spaces between the particles of sand, have been manufactured for fifty years or more. Sand brick with calcium silicate as the cementing, bonding and filling material, does not date back more than ten or twenty years. In the latter case, there is a chemical combination or reaction between the two constituents forming the brick, which binds the two together in a manner akin to vitrification. The resultant product is similar in many respects to dry press brick, in that vitrification does not penetrate to the core of each particle. In the former this is not the case.

For either product the necessary raw materials are the same—sand or a granular silicate and lime, (either a high calcium lime or a dolomite lime). The method of treatment during the process of manufacture differentiates them into three distinct products, all of which go indiscriminately under the same name.

SAND BRICK WITH EXCLUSIVELY CARBONATE FILLER.

It is obvious that the stone which has carbonate of calcium as a filler or binding material cannot have the same strength as the one in which there is a chemical union of the sand and the calcium or magnesium compound, as the case may be. However, the former can be manufactured at a less cost, and probably fills certain requirements in building construction. Therefore, we will give it brief consideration.

Twenty to forty percent of completely slaked lime is thoroughly mixed with sand and sufficient water to make moulding possible at moderate pressures. The hardening process is carried out in one of three ways:

First. The brick are exposed to the action of the atmosphere for a long time, the caustic lime thus slowly acquiring the needed carbon dioxide ($CaO + CO_2 = CaCO_3$). The necessity for complete slaking is evident, since any unslaked lime would slake in the air and rupture the brick. The time required before they can be used is five to six months, and they attain their maximum strength in about one and one-half years. The hardening here is similar to that of mortar, and like mortar is only superficial. The best lime for this purpose would naturally be a hydraulic lime. Zwick has said that only thirty per cent. of the lime in very old mortars, had been converted into the carbonate.

Second. Hardening in an atmosphere, rich in (CO_2) carbon dioxide, without pressure. This gives the same results as the first process, in less time.

Third. Hardening with (CO_2) carbon dioxide under pressure. It is claimed by some that this means gives a product in which nearly, if not all the calcium hydroxide $Ca(OH)_2$ is converted into carbonate. However, I do not believe that this has been satisfactorily proven.

SAND BRICK WITH PARTLY CARBONATE AND PARTLY SILICATE FILLER.

In this process the treatment up to the hardening process is essentially the same as just described, but the hardening is effected in a warm, moist atmosphere, saturated with CO_2

from the lime kilns, and the heat of slacking lime is used to aid in keeping the temperature up to 60–70 degrees C. A combination of the methods for class 2 and 3, provides for the introduction of CO_2 into the kettles or closed iron cylinders used for hardening in class 3. Under the conditions first cited, the product has a binding material for the most part $CaCO_3$, but some hydrated calcium silicate is probably formed, thus strengthening the bond. In the latter, the binding material in mainly the calcium hydrosilicate, and the amount of calcium carbonate much less than the former.

I am inclined to the belief that here also the superficial hardening prevents anything like a nearly complete change of the $Ca(OH)_2$ in the center of the stone. This of course would be no detriment to this product, or the preceding one, except to give a weaker brick, *provided only* that the surface had reached sufficient hardness and density to prevent all access of water or gases. If this is not obtained, we should expect effloresence as a result of the solubility of the calcium hydrate.

SAND BRICK WITH HYDROSILICATE FILLER.

This brings us to the third class, the only one which I think will ever take any important place in the world's markets. Therefore, we will go into the essential facts more closely.

The Sand.

The same raw materials are used as in class 1 and 2, viz. sand and lime. Considering the sand first, the following facts have been collected.

First. Almost any sand *can* be used, and a fair product produced, if the treatment is properly varied to suit the physical and chemical properties of the sand. Economy in manufacture, however, limits both physical and chemical properties.

Second. A comparatively pure sand is essential to cheap manufacture,—nor should the sand be too coarse. It is probable that for the best results most of it should pass through a screen, 20 meshes to the inch, unless there is a

good gradation from extremely fine to the coarse sizes, in which case perhaps coarser sand could be worked in to advantage. The product would not be as smooth and pleasing to the eye as one made from finer sand.

The object is to obtain a sand which will contain sufficient very fine particles to combine with the lime, and which when pressed will leave the smallest possible interstitial spaces, thus reducing the amount of lime necessary for welding these particles together.

Work done in connection with the cement industry has shown that practically none of the sand coarser than 100 mesh, and only a part of that of 150 mesh enters into the active cement reaction.

As an illustration, suppose we had one sand all of the size of walnuts, and another all of the size of wheat. Enough of the smaller could be mixed in to materially reduce the interstitial space, and yet leave sufficient space for a binding agent.

The finer the material, as a whole, the more difficult will it be to so mix it as to get a coating or film over each particle of sand. On the other hand, the union will be more complete, and the product more pleasing to the eye.

Experiments carried out by Prof. M. Gasenopp in the chemical technical laboratory of the Polytechnic Institute at Riga, the results of which were published in the Thon Industrie Zeitung for October 1900, Vol. 24, p. 1703 and Vol. 25, p. 762, clearly show the necessity of using fine sand for rapid and economic production.

The first set of results were obtained with fat lime as the active agent, and the second with dolomite lime.

In both cases a very pure sharp coarse sand 0.6 to 1.0 mm. in diameter was used, and a fine sand, 0.2 to 0.3 mm. in diameter. The fine sand was somewhat rounded and contained a considerable quantity of soluble silicic acid, therefore it was treated with HCl, and caustic soda solution, and washed before using in the experiments.

Each size of sand was treated with 10 percent and 20 percent of lime respectively, and with 10 and 20 percent of dolomite lime. High pressure steam was used. Both sets

were run for eight hours, one at 5 atmospheres pressure, the other at 10 atmospheres.

The tables of analyses of the resultant products given below speak for themselves. The soluble silicic acid is an index to the extent of the reactions. The methods of analysis are not given. The samples were exposed to the air for two to five weeks, which probably accounts for most of the CO_2 since the $Ca(OH)_2$ was not all converted into the silicate.

[See Tables I and II, pages 161 and 162.]

Considering the above table, we see that the coarse and fine sand was very differently affected under the same conditions, viz.:

TABLE III.

Percentage of Caustic Filler Used	Pressure of Steam in Atmospheres	Percentage of Soluble Silica found by Analysis of Product		
		Kind of Caustic Filler Used	Using Coarse Sand	Using Fine Sand
10	5	Pure Lime	0.48	8.06
		Dolomite	0.75	1.98
	10	Pure Lime	8 33	7.58
		Dolomite	8.14	6.29
20	5	Pure Lime	0.59	3.41
		Dolomite	0.65	....
	10	Pure Lime	2.75	11.14
		Dolomite	1.69	7.03

In every case the reaction is much more complete with the fine sand. This shows clearly the necessity for some fine quartz. The reaction has gone a great deal farther with the fine than the coarse, although the fine sand was not in as favorable physical shape for chemical union as the coarse. If soluble silicic acid were naturally in the sand, it might take the place of fine sand. In each case, the lime was not all combined with SiO_2, that is the reaction was incomplete, as is shown by the presence of uncombined $Ca(OH)_2$. After

TABLE I.

Series A. Brickettes in which pure lime was used. Analyzed after eight hours exposure in steam cylinder.

No.	Mixture	Stean Pressure in Atmosph're	Total CaO	$Ca(OH)_2$ from Total	MgO	Al_2O_3+ Fe_2O_3	CO_2	Chemically Combined Water	Soluble SiO_2	Sand	Total
1	90 parts coarse sand..... 10 " lime.....	5	6.50	1.67	....	....	3.45	0.88	0.48	89.10	100.37
2	Same as No. 1	10	9.38	1.24	....	0.42	2.54	2.96	3.38	82.00	100.68
3	80 parts coarse sand..... 20 " lime..	5	16.69	7.16	....	....	4.77	2.78	0.59	74.67	99.50
4	Same as No. 3	10	16.59	6.05	...	0.29	3.02	4.60	2.75	72.85	100.10
5	90 parts fine sand..... 10 " lime.....	5	8.74	0.85	....	0.31	3.75	1.79	3.06	82.02	99.67
6	Same as No. 5	10	10.10	0.58	....	0.56	2.23	2.55	7.58	76.88	99.40
7	80 parts fine sand..... 20 " lime.....	5	15.81	4.23	....	0.31	5.21	3.90	3.41	70.68	99.32
8	Same as No. 7	10	17.67	0.98	....	0.65	3.13	3.59	11.14	63.05	99.42

TABLE II.

Series B. Brickettes in which dolomite lime was used. Analyzed after eight hours exposure in steam cylinder.

No.	Mixture	Steam Pressure in Atmosph're	Total CaO	$Ca(OH)_2$ from Total	MgO	Al_2O_3+Fe_2O_3	CO_2	Chemically Combined Water	Soluble SiO_2	Sand	Total
1	90 parts coarse sand 10 " dolomite	5	4.98	0.88	3.81	0.23	2.21	2.24	0.75	85.23	99.45
2	Same as No. 1	10	4.49	0.43	3.45	0.96	2.05	2.13	3.14	83.61	99.83
3	80 parts coarse sand 20 " dolomite	5	8.56	2.53	6.58	0.70	3.38	3.44	0.65	75.80	99.11
4	Same as No. 3	10	9.97	1.92	7.57	0.95	3.19	4.39	1.69	72.62	100.18
5	90 parts fine sand 10 " dolomite	5	4.97	0.92	3.75	0.68	2.22	1.82	1.98	84.23	99.65
6	Same as No. 5	10	5.40	0.40	4.07	0.95	1.92	1.88	6.29	79.28	99.79
7	80 parts fine sand 10 " dolomite	10	9.23	0.79	7.00	0.94	1.82	5.74	7.03	67.88	99.64

taking from the chemically combined water that which would go with the $Ca(OH)_2$, the balance of the water gives some striking molecular ratios to the soluble silicic acid. Thus from Table I we have

TABLE IV.

Series A.

Number	1	2	3	4	5	6	7	8
Ratio	3.7	2.66	6.50	3.7	1.7	1.	2.8	1.

Series B.

Number	1	2	3	4	5	6	7	....
Ratio	9 55	2.16	14.4	7.4	2.7	0.98	2.60	....

The ratios 6.5, 7.4, 9.55 and 14.4 seem a little unusual to me. It is notable, however, that with one exception, the high ratios are with the MgO.

These results also show clearly that high pressure is essential to anything like rapid working.

Professor Michaelis is quoted as saying the higher the pressure, the higher the soluble silicic acid, and the solider and denser the resultant brick.

Professor Michaelis has given the physical properties of the sand primary importance, and chemical properties a secondary place. He also says that two-thirds coarse and one-third fine sand give a denser and firmer brick than where the sand is all of one size. 70 per cent. coarse sand, 30 per cent. fine sand, with 5 per cent. added lime gives a good product, having 7 to 8 per cent. porosity. He has also shown that good brick can be made with sand carrying as much as 40 per cent. clay, if sufficient lime is used.

Judging from his figures, I should say for each part of clay present a little less than one part of lime must be added in excess of that which would be needed for the sand itself. While 2 to 3 per cent. of lime is probably all that is necessary to effect a satisfactory bond, in practice more is used, in order to hold the sand together and to facilitate pressing into a form which can be handled.

In conversation with Mr. H. E. Brown, of Coldwater, Michigan, recently, he said 2½ per cent. of kaolin added to a mixture of dolomite lime and sand had given him tensile strength higher, if anything, than brickettes made from same material without the kaolin. On the contrary, it is claimed by others that the clay has a tendency to reduce the hardness, and make the brick more susceptible to weathering action.

From theoretical considerations we might say the presence of silicates in any large quantity would act as a dilutant or inert material with perhaps a few exceptions. Kaolin (Al_2O_3, 2 SiO_2, 2 H_2O) may be converted into anorthite (CaO, Al_2O_3, 2 SiO_2). But we do not know this to be the case. Also anorthite, if present in the sand might take on four molecules more of SiO_2 and five molecules of H_2O as water of hydration and form heulanite (5 H_2O, CaO Al_2O_3, 6 SiO_2), and the acid orthosilicates represented by phenite (H_2O, 2 CaO, Al_2O_3, 3 SiO_2) might take on a molecule of CaO and give up a molecule of H_2O.

It is hardly likely that the normal silicates would become more basic in the presence of large quantities of available silicic acid. It is not likely that the metasilicates, represented by the simplest silicates of calcium and magnesium, enstatite (MgO, SiO_2) and wollastonite (CaO, SiO_2), can do more than promote crystallization of the calcium or magnesium silicates formed, by their mere presence in crystal form.

The silicates containing the alkalies, potassium and sodium, are probably barred on account of yielding soluble salts. From silicates of the heavy metals, the oxides would be thrown out. At any rate, they are too valuable for other purposes to be considered here.

Since under the working conditions, the very resistant quartz is brought into the reaction, the greater part of the complex silicates are very likely to be broken up into more simple forms, in most cases at least yielding inert compounds in part.

At any rate it is evident that when silicates are present in any quantity, more lime will be required, and while it might be possible to make a good brick from a silicate sand, it would not be a good business proposition.

As an instance of the weakening action of inert materials, we might cite the slightly lower crushing strengths as shown in a table given later.* Mr. H. E. Brown also confirmed this observation, finding the tensile strengths less on colored brick than on the same brick without the coloring matter, when treated in exactly the same manner.

Hence it follows that the sand should be comparatively pure, made up of medium coarse with some very fine grains. Sharp sand is preferable so as to present thin sharp corners to promote chemical combination. The sharp sand will also resist change of position or flow of solid particles better than rounded grains. If the sand available contained any large amount of clay it would probably be economy to roughly wash it. And if it did not contain sufficient very fine particles, a portion would have to be ground. This would increase the cost about five to seven cents per 1000 brick produced.

The Lime.

The prime requirement is proper burning. Since dead burning of limestone or dolomite tends to produce a crystalline oxide, which slakes or hydrates with extreme slowness, a portion might remain unslaked and uncombined, and make trouble by future slaking, and possible change of volume and possibly effloresence. It would hardly rupture the brick, since the crystalline oxide is said to hydrate without much change in volume.

Economy, however, indicates the use of the purer limes (lime, dolomitic lime or dolomite lime). The impurity is usually clay or sand, and at the temperature at which limes are burned, the SiO_2 will be combined with the lime, and the Al_2O_3 remain as such, both acting as inert bodies so far as sand brick are concerned.

German practice up to the present time seems to be to use only the fat limes. Ernst Stöffler, an engineer of Zürich, Switzerland, who has published a pamphlet on this industry, says fat limes are the best. Prof. Glasenapps' work, early in 1901, has shown that essentially the same results obtain when working with dolomite lime as with the fat limes.† I see no reason why dolomite lime should not be

*See Table VI. †See Table I.

a desirable material, except that it will require more care and time to properly slake it. We would have the increased fluxing action due to two bases instead of one. Even if the over-burning, which the MgO is bound to suffer before the CaO can be properly burned in a dolomite, should cause it to slake so slowly as to go through the process unchanged, it does not offer the same disadvantages that unslaked CaO would, since the oxide requires 1000 times its weight of water for solution, and the hydrate is practically insoluble. However the unslaked MgO would probably slake when exposed to the hot steam, and in that event might expand and rupture the brick in the hardening cylinder.

The dolomite lime may, however, give a brick which will be slightly lower in fire resisting quality than one made from the fat limes, especially if the amount of lime used is high. If this should be the case, it would not be due to the lower melting point of $MgSiO_3$ but, to the greater activity of two bases acting in unison on an acid.

PROCESSES OF MANUFACTURE PRIOR TO HARDENING.

Present practice adopts one of four methods prior to hardening: 1st. The wet slaking process. 2nd. The dry slaking process. 3rd. The acid slaking process. 4th. The quick lime or oxide process.

First Method.

This process consists in slaking the lime to a fat putty, either by hand or by mechanical aids, and then mixing in the desired proportion of sand and water, either in a wet pan or a large, slow moving pug mill. From here it is carried to bins over the press, allowed to stand a short time to ripen and then pressed.

With properly burned fat limes, the proper amount of water and sufficient agitation, this is accomplished very quickly and thoroughly. The heat given up by the union of CaO and H_2O is sufficient to generate steam in the minute pores of the amorphous oxide, and thus break it up into the smallest possible particles, constantly and rapidly exposing new and fresh surfaces to the hot water. CaO on hydration

gives up 270 heat units. Under these conditions the CaO seems to form a hydrate, carrying more than one molecule of water, the excess being locked in so loosely that a little excess of heat, as would be the case with too little water, would prevent its formation, or break it up if formed. Water acts as an equalizer; not enough allows it to get too hot, and too much cools it too low for the best results.

Walters found that the best slaking of high calcium lime gave an increase of volume of 3½, air slaking 2½, and slaking with a large quantity of water $1\frac{7}{10}$. This increase in volume gives it greater spreading or enveloping power.

With the dolomites more time is required for proper slaking, for two reasons: The MgO is somewhat over-burned, and second, the heat units evolved by MgO on hydration is much less than that for the hydration of CaO. Both act in the direction of retardation.

The process can be much improved and quickened by the use of hot water or the application of external heat.

It is claimed by many that the magnesian limes never develop the same degree of plasticity as the fat limes. This may in part be due to imperfect slaking.

Second Method.

This differs from the preceding only in this, that the lime is slaked with just enough water so that the heat of the chemical reaction shall just dry the finished hydrate. The hydration is incomplete in most cases. There are a number of patent machines for doing this, both in this country and in Europe. Some are closed tight, and the slaking done under pressure with water alone, others are treated with both water and steam.

In another type, water is added to the lime in a closed vessel, and heat applied externally by means of a steam jacket to the container.

Such hydrates of this class as I have examined, have been found to be very imperfectly hydrated. However, none were examined when water was added and steam supplied to keep the mixture hot, or where external heat was used.

Mr. H. E. Brown claims that dolomite lime hydrated under the conditions last mentioned in a steam jacketed

machine, showed on chemical analysis complete hydration, with a three-fold increase in volume, and yet a dry product.

Third Method.

Five to ten per cent. of a solution of commercial hydrochloric acid—19 degrees B., is added to the lime after slaking has begun. This is the German patent of Peter Kleber. (No. 103777.) This acid is worth about one to one and one-half cents per pound. In this process the preliminary treatment only carries the hydration to from one-third to one-fifth of completion.

This of course gives a small amount of $CaCl_2$. The affinity of this compound for water is well known. It is also very active in attacking silicates, as is illustrated by its action in the nascent condition in the J. Lawrence Smith fusion for the determination of alkalies.

The introduction of the acid must be objectionable at any rate, since if the $CaCl_2$ remains as such, it would be soluble later, and give trouble. If it is decomposed, and $Ca\ SiO_3$ formed, the acid liberated in the warm moist atmosphere must certainly attack the metal cylinders unless it is glazed with a silicate of some sort or lead lined.

$$CaCl_2+SiO_2+H_2O=Ca\ SiO_3+2\ HCl.$$

It is claimed by Kleber, that the HCl accelerates the slaking and shortens the time needed in the hardening vessel. The introduction of $CaCl_2$ is not so satisfactory as the use of the acid. While there are a number of plants operating under this patent, I do not believe that much acid is now used.

Fourth Method.

The quick lime or dry oxide of calcuim is mixed with the sand, and just enough water to make it possible to work it in the press. The water is added in two portions, with a short interval of time between additions. The product then goes to the press.

This certainly is the most rational and rapid method, if as good a product can be produced as by other processes. This process probably requires a little more lime than the others in order to get the mixture in proper condition to

mold. Where the cost of lime is very high, this would be an item. But much may be done to reduce this lime by mixing with it a portion of extremely fine ground sand.

If lime hydrates with steam under pressure in a crystalline form without change of volume, as Walter's experiment in hydrating lime in a glass tube with steam seems to show, the hydration during hardening would not in any way injure the brick.

Statements made to me by Mr. Brown, however, indicate that for dolomite there is a large increase in volume. He used water and steam both. My own experience points to the fact that both CaO and MgO will expand when treated with steam. If such be the case the fourth method would not be feasible.

It is also possible that the hydro-silicate of calcium may form direct from the CaO without change in volume. If this were the case, we should work with as little water as possible to get the denser brick.

HARDENING PROCESSES.

The hardening is accomplished in one of two ways, either by low pressure steam and long exposures, or high pressure steam and short exposures. In the first case, the pressure used is about two atmospheres, giving a temperature of 120 to 125 degrees C; and the duration of the exposure is about seventy-two hours; in the second case, the steam pressure is from eight to ten atmospheres, giving temperatures around 185 degrees C, and the exposure lasts for a few hours only, six to ten hours being sufficient in most cases.

The letters patent on this latter and most important procedure were taken out by Professor Michaelis early in the eighties, and permitted by him to lapse without commercial application.

I have already mentioned a modification of the hardening process in connection with class No. 2, in which CO_2 is introduced either with the steam or after it. If this method is used at all, it is usually when working with low pressure steam.

According to Stöffler, the quantity of lime used in any of the methods varies from 5 to 10 per cent. And the moisture above that used in hydration, from 7 to 9 per cent. The nature or the make up of the sand, whether quartz or feldspathic or clayey, will determine the amount of both lime and water desirable.

From the foregoing statements you can readily conceive that it would be entirely feasible to take the sand from the bank and have a brick ready for market in less than a day of twenty-four hours, or if you include the burning of the lime, the entire process from the raw limestone and sand to the finished product need not consume more than thirty-six hours.

QUALITY OF THE PRODUCT.

Concerning the quality of this new artificial stone, the question arises in the mind of each of us: "Is it of any value to us?" "Will it fulfill our requirements for a safe and desirable building material?" I think it will, and offer the following as evidence of its quality:

The sample before you is one made in Germany, by the Stahl and Eisen Actien-Gesellschaft, Hoerde, Germany, from materials which you will see were not the most desirable. An analysis by rapid methods gave;

84	per cent.	SiO_2 (sand and Sol. SiO_2).
2	"	$Fe_2O_3+Al_2O_3$
7	"	CaO
	"	MgO low.
93.	per cent.	
7.	"	Balance is made up of MgO, H_2O and CO_2 and Alkalies.

When three grams were ground fine and treated with a large amount of water, only a trace of Ca was found in the water.

The water absorbed on standing over night is 9 per cent. The question naturally is, "Will it stand weather?" The small piece was soaked in water, frozen and thawed three times. It was thawed out the last time on the laboratory hot plate at about 250 degrees C. I could observe no

evidence of damage by frost, except when it froze fast to a sandstone window-sill, and broke out a little when I wrenched it loose. After thawing out, and while still wet, I set it up and turned a blast lamp directly onto one small piece of it, and brought it up rapidly to a bright red heat, and held it for fifteen minutes, then plunged it into cold water. This last proceedure destroyed the bond, as you can see by rubbing the brown spot on the brick. A block sawed from this brick and crushed, showed a crushing strength of 4210 pounds to the square inch.

Further in a paper given by Herr G. Beil, of Aschersleben, at a meeting of the Kalksandstein society in Berlin, March, 1901, the following results are given as coming from The Köenigl. Mechan-technichen Versuchsanstalt, at Charlottenburg, Germany.

Seventy brick, representing 7 different products, were soaked in water at 12°-15° C., frozen for four hours then placed in warm water three hours. All were pronounced sound at the end.

Also the following list of tabulated results (published in Vol. 25 Thon-Ind. Zeit. 575):

TABLE V.

Number of Test		Absorption	Crushing Strength, in pounds per square-inch		
		Amount of water taken up by dried sample, after 125 hrs. soak, in percents of dry weight.	Water soaked Brick	Dry Brick	After Freezing
1	High Pressure Hardening	12	1903	1704	2229
2		14	2073	2215	2300
3		9	3933	4189	4260
4		10.6	1846	1732	2187
5		——	——	3710	3288
6	Low Pressure Hardening	——	——	850	1385
6a		——	——	1353	1747
7		18.3	1335	1193	1562

This shows that the crushing strength is not, in most cases, very materially decreased by freezing. Crushing wet also produces a slight decrease.

In the Thon-Ind. Zeit. Vol. 25, 1660, the following tabulated results are to be found:

Swedish brick, tested at Gothenberg by same method as in above Table V.

TABLE VI.

Kind of Brick (All Lime-Sand)	Specific Gravity in Powder Form	Actual Specific Gravity	Percentage Absorption	Porosity in Percents of Volume	Number of Brick Tested
1. Smooth, gray-white	2.51	1.74	13.1	30.5	10
2. Yellow-brown to red	2.46	2.10	13.6	14.6	10
3. Smooth, gray-white	——	——	——	——	4
4. Brown-red	——	——	——	——	4

	CRUSHING STRENGTH								
	Dry			Wet			Frozen Repeatedly		
	Max.	Min.	Mean.	Max.	Min.	Mean.	Max.	Min.	Mean.
1. Same as above	17.32	12.21	15.48	15.90	10.37	13.21	14.91	7.24	11.98
2. Same as above	14.63	7.53	11.08	14.48	6.36	9.80	11.64	9.23	10.08
3. Same as above	27.41	15.19	22.86						
4. Same as above	21.58	12.35	15.62						

I do not know the process of manufacture.

This again shows a slight decrease in crushing strength after repeated freezing.

Mr. Brown has obtained tensile strengths as high as 650 pounds, but gives as an approximate average for ten per cent. of the hydrates 400 to 425 pounds. He thinks the maximum tensile strength is obtained with very fine sand and about fifteen to twenty per cent. of the hydrated dolomite lime equivalent to nine to thirteen per cent. of the oxides.

A cube 2x2x2 made by Mr. Brown from finely ground sand and dolomite lime, gave a crushing strength of 5730 pounds to the square inch when crushed in the mechanical laboratories at Ohio State University.

The following is a copy of tests made on brick manufactured by H. Huennekes:

Tests of Lime—Sand Building Brick.

Made for A. G. Roenick, 408 Wood Street, Pittsburgh, Pa., by the Pittsburgh Testing Laboratory, October 17, 1901:

Crushing Strength Per Square Inch	Absorption Test 45 Hrs. in Water.	Absorption After Freezing Test	Crushing Strength Per Square Inch After Freezing
3518 lbs.	11.6 % gain	12.4 % gain	*4137 lbs.
4162 lbs.	8.57 % gain	8.8 % gain	*5202 lbs.
3859 lbs.	11.8 % gain	11.4 % gain	

*Bricks did not crush at this load, but showed only slight cracks at an actual load of 105,000 pounds, the limit of the machine.

Half bricks were used in absorption tests; these bricks were dried thoroughly before immersing in water. After remaining in water forty-five hours, bricks were frozen four hours at a temperature of 14° Fahr., and then thawed in warm water twelve hours, frozen again at a temperature of 9° Fahr. for a period of three and one-third hour, thawed in hot water three hours, frozen at a temperature of 12° Fahr. for three and one third hours, and finally thawed in hot water for twelve hours. Final absorption test made and then bricks were again thoroughly dried. The bricks showed no signs of cracking or disintegration.

From the foregoing, it is evident that any of the high pressure steam process stones have strength to meet all ordinary requirements for building purposes. In Germany a great many of the fire inspectors have pronounced them a safe material from their standpoint, and a good many sand-brick have gone into municipal and government buildings over there. The railroads are using them also.

I have taken the following from Thon-Ind. Zeit. V. 25, 1822:

A small oven was built of brick made by high pressure steam hardening, by Guthemann, of Niederlehme, some whole and some bats. It was fired by wood up to a temperature of 1100 degrees C, measured both by Seger cones and

by electric pyrometer. The stones were not injured in any way by this process, and further showed that they were excellent non-conductors of heat.

Later, by means of a blast, the furnace was brought to a glowing heat and the hose turned directly on to the hottest part with a full stream. This cracked off a few corners. When thoroughly cooled it was found that the bond had been destroyed for a depth of 3 cm.= ¾ in., so that part could be scraped off with a knife.

Mr. G. Beil, Thon-Ind. Zeit, 25, 577, cites the following good qualities:

1. Great hardness.

2 and 3. Regularity of form which permits of more rapid and consequently cheaper building.

4. The beauty of the structure.

5. Outside dressing or painting unnecessary.

6. Inside plastering unnecessary in many cases.

7. No insulation necessary.

8. It is cheaper than cut stone or terra-cotta.

9. Cheaper than brick. The costs in Berlin are: Ten to twenty marks per 1000, for sand-lime brick; seventeen to twenty-five marks, per 1000, for face brick. Of course eight and nine represent German conditions, not American ones.

MECHANICAL EQUIPMENT FOR MANUFACTURE.

As has been intimated in previous statements, the machinery in a general way consists of apparatus for either wet or dry slaking, conveying machinery, mixing machines, presses, trucks, hardening boilers and machinery for generating power.

There are a large number of patented machines for the dry slaking of lime, both in this country and in Europe. I know of but one, however, for wet slaking. In this latter process, the lime is slaked with sufficient water to make a good putty, and after maturing in this condition, it is dried by artificial heat and ground.

Since in the various patented processes for making sand brick, the only claim which, in most cases, could be con-

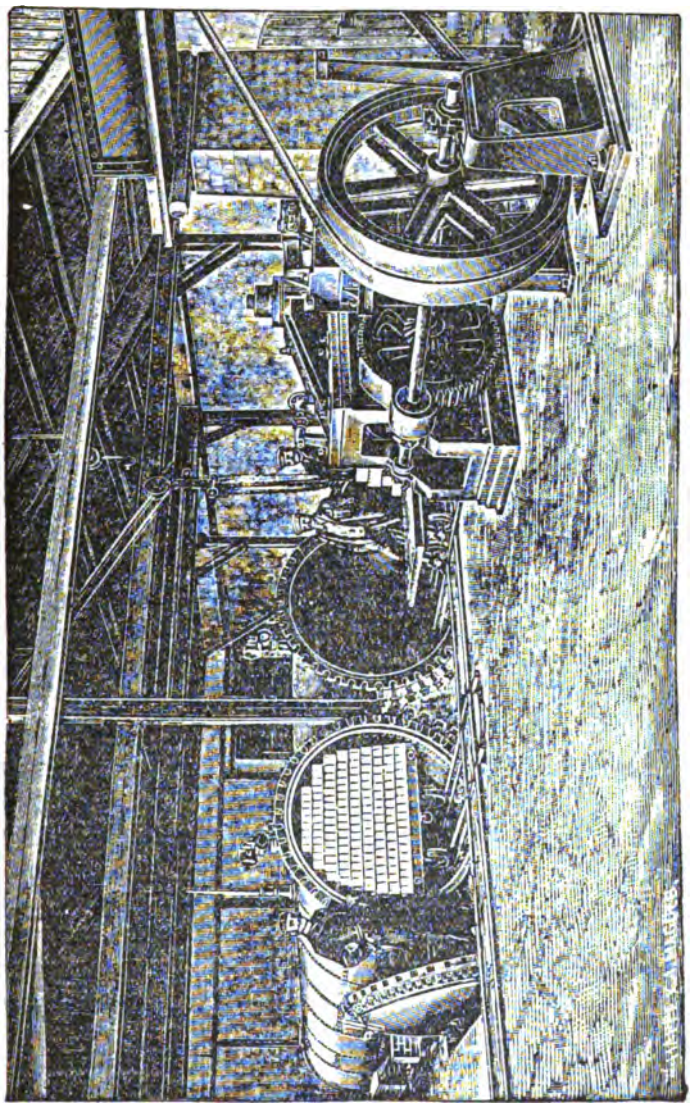


Figure I.

tested, is that for some special machine or method of slaking lime to a dry powder. I will briefly mention a few of them:

Olschewsky, in one of his German patents, uses a cylinder capable of resisting strong pressure, so mounted that it can be rotated, and receive a supply of water at the same time. The cylinder is filled one-half full of CaO, and sealed up. Rotation and the introduction of water begins: rotates one-fourth to one hour, and allows to stand two or three hours. Thirty to thirty-five parts of water are used to one hundred parts of lime.

From what I have seen in this country, I feel safe in saying such a machine will give imperfect hydration.

Another of Olschewsky's patents, German No. 82785, consists in the the building of a tight metal box in the bottom of the trucks for carrying the brick in the hardening chamber. This is provided with a stirrer, a shaft with peddles. Water and steam are introduced. The heat generated serves to dry the green brick. This wagon then is run into the hardening chamber, and you always have one day's supply of slaked lime ahead.

The German patent, No. 82785, was recently annulled.

This device or proceedure is patented in England, 1900, No. 1705, and in the United States, Dec. 1900, No. 663459.*

There are several other American patents using various forms of agitators.

Another we might mention is that of A. G. Schwarz, of Zürich, Switzerland, in which the machine is heated by steam pipes and the hydrate is dried by producing a vacuum, as all excess of water is pumped out as vapor. This machine is illustrated and described in "Silico-Calcareous Sandstones," by Ernst Stöffler.

Mr. H. E. Brown holds patents which claim for dolomite lime everything which is claimed by the Germans for lime.

It is apparent that the best conveying machines will be those which will move the material with as little friction between the conveyor and sand as possible, on account of the latter's strong abrasive properties. Broad continuous

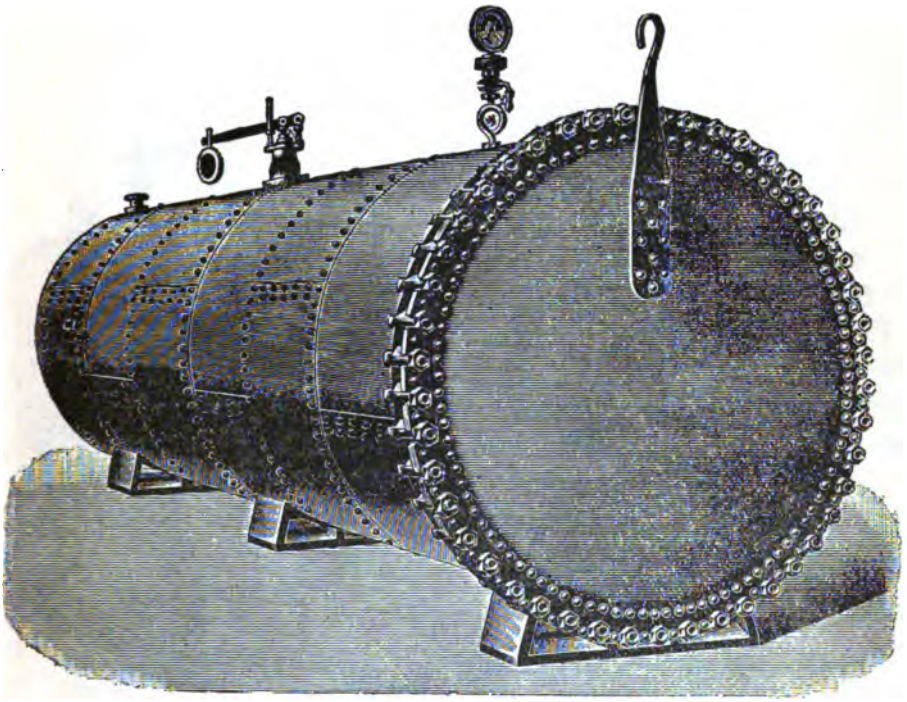
*See Patent Office Gazette, Dec., 1900, page 1946.

belts, where feasible, or heavy chain bucket conveyors are perhaps best adapted to this purpose, where dump cars are not desirable.

If screws or auger machines are used for conveying or mixing, they should be of large diameter and capacity, and geared to move slowly.

Edge runners are much used in mixing and grinding.

Figure 11.



The trucks are usually built low and made to carry from 1,000 to 1,400 of brick of American size, or fewer of German size. They usually carry thirteen to sixteen courses.

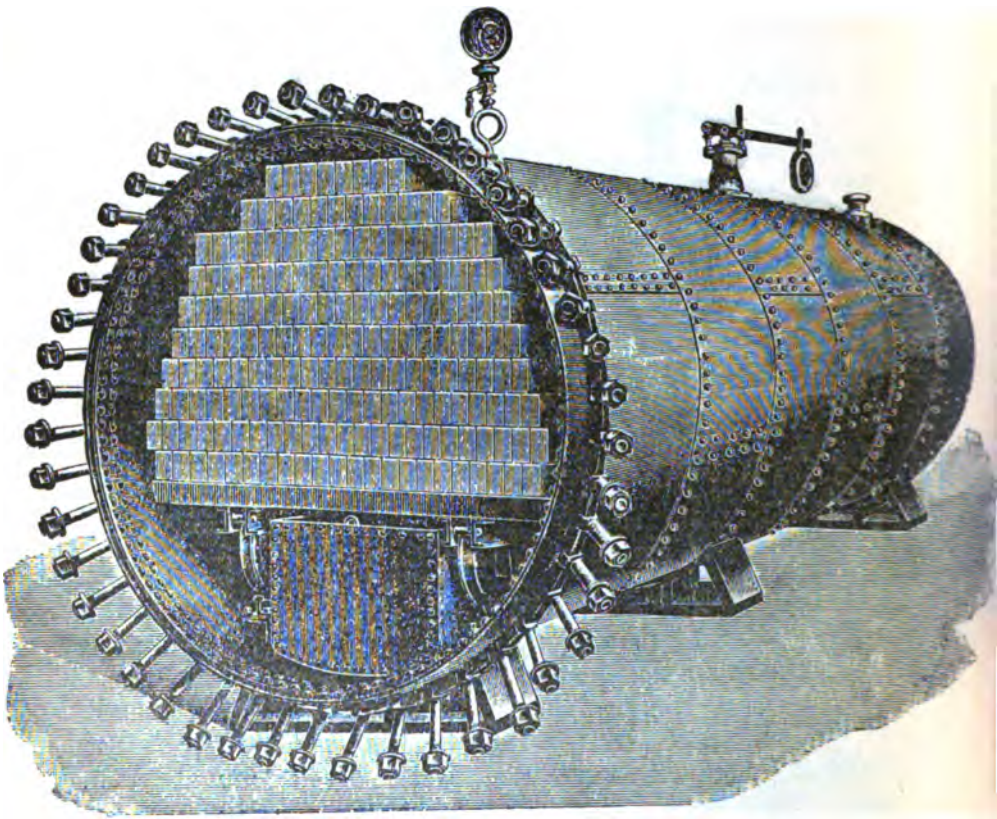
The hardening boilers are made of iron or steel, and constructed to stand for high pressure — 150 to 200 pounds pressure to the square inch. They should always be bricked in to prevent rodiation and excessive condensation of steam. They usually have one head movable and bolted on. This head is handled by a small crane.

The customary size is six to seven feet in diameter by fifty to 60 feet in length.

Figure II shows a hardening boiler closed.

Figure III, a similar boiler open and charged ready for steam as soon as the head is bolted into place. This plate also shows a container for slaking lime, under the floor of the truck.

Figure III.



There are a number of mixing machines in use — wet pans, auger machines, pug mills, &c. One of rather unique type is that of A. G. Schwarz. It consists of a drum with a mixing arm. A cut of this machine may be seen in "Silico-Calcareous Sandstones." It is arranged so as to remove

moisture by producing a vacuum, and then add a definite amount of water, working on definite amount of material. It is necessarily intermittent in its action.

Although an expensive way of operating, it has some things in its favor, since the amount of water cannot vary widely for any given mixture. The amount of water which is best must be determined for each plant.

When once this is determined the Schwarz machine offers a means of always getting that amount into the material.

COST OF PLANTS AND PRODUCTION.

Ernst Stöffler, of Zürich, in his pamphlet gives the following for a plant using quick lime. The cost of a plant and of production will not vary widely from this for any of the others.

PLANT, 40,000 DAILY CAPACITY.

Land and Buildings.

	About 5000 m_2 of land, of which about 1500 m_2 is built over, machine masonry, chimney, tracks silos, etc....	$24300 00

Machinery Plant.

A.	Preparation of the lime: One stonebreaker, conveying screw with elevator, one ball-mill, tilting waggons.....	2382 80
B & B_1	Mixing machines: Two elevators, two intermittent-charge mixing machines, wagons, two continuous mixing machines, moistening apparatus, two elevators.....	4082 40
C.	Press room: Two endless belt conveyors, six mixing machines, six presses, wagons, moveable platform.....	14774 40
D.	Hardening room: Three hardening boilers with pipes.....	7776 00
E.	Boiler: Two steam boilers	5054 40
F.	Engine house: Steam engine.	2916 00
G.	Shafting and pulleys.....	2916 00
H.	Lighting.....	1166 40
I.	Unforseen, sundries.....	2721 60
		$68040 00

To bring the machinery to this country, excluding hardening room and power machinery, would cost an import duty of 45 per cent. of the German valuation; therefore, we have

A	$ 2,332.80
B	4,082.40
C	14,774.40
Total	$ 21,189.60

Duty = $9,535.03 — and freight and transfer charges will bring this up to $10,000, so we would have such a plant costing in this country about $78,000 equipped with German machinery. This alone would add eight cents per thousand to the cost of production.

Figures for cost of production B:

AMERICAN CONDITIONS OF PRODUCTION.

Sand — 157 cubic yards @ 7 cents.....	$ 11 00
Lime — 13 cubic yards = 11 tons @ 4.00 per ton — ($3.00 if you own your own stone and burn it yourself)..	44 00
Coal — 3 tons @ 2.25.....	6 75
Repairs.....	5 00
Oils, etc.	3 00
Forty workmen @ 1.35.....	54 00
Foreman @ 2.50.....	2 50
Office expenses.....	20 00
Depreciation and interest, 10 %.....	20 00
	166 25
Selling expenses — about 10%.....	16 00
	182 25

According to the estimate, the brick could be put on the market without profit at 4.55 per 1,000 when machinery is bought in Germany and with a plant costing $80,000. However, there is no necessity of importing machinery.

A plant with American machinery will cost about, as follows:

A liberal estimate for land and buildings, I think, would fall well within.....	$15,000 00
One wet pan (30 HP).....	1,000 00
One Ball mill (5 HP) or other arrangement.....	500 00
Two presses	4,400 00
Two pug mills (20 HP)..... ...	800 00
Conveying machinery.....	6,000 00
Shafting and belting	3,000 00

One 100-HP Corliss engine	2,500 00
Two 100-HP boilers	2,000 00
One 25-HP boiler	800 00
Four hardening boilers, 7 diameter by 60 long	8,000 00
Placing same and insulating same	1,000 00
Pipes for preliminary heating	1,000 00
Railroad tracks and unforeseen expenses	4,500 00
	$50,000 00

I think a plant satisfactory in every way could be installed for $50,000, and brick produced for $4.00 per M., ready for market, if the plant was favorably located.

At any rate, it is safe to say that this product can be put on the market as cheap as pressed brick, since the losses in manufacture, in a plant in good running order, do not exceed two per cent.

I have purposely made estimate of cost of plant rather high, since with any young industry there is more or less delay and expense in getting everything properly adjusted. The time consumed and amount of this expense of course will depend on the ability of the men in charge.

I have used the figures given by Mr. Stöffler since they seemed to represent about the average of several estimates which were available for comparison.

In the back of Mr. Stöffler's book, "Silico–Calcareous Sandstones," are a number of plates giving plans of various sand brick plants.

The plans here published were furnished by Mr. Fred C. Jenkins, of Hamburg, Germany.

The plan of plant here given is not one which would meet our requirements, but is given to show what sort of a place the sand brick plants now operating are. The illustrations were copied from advertising matter, because they show types now in use.

I take this opportunity to acknowledge my indebtedness to the following persons for much valuable information: Stahl and Eisen Actein Gesellshaft, Höerde, Germany; C. Luke, Eilemburg, Germany; Fred C. Jenkens, Hamburg, Germany; Ernst Stöffler, Zürich, Switzerland; Sandstein-ziegelei-Patente Schwarz, A. G., Zürich, Switzerland; Brüch Kretschel & Co., Osnabruck, Germany.

One plant has been started in this country and two more are contemplated soon.

DISCUSSION.

The Chair: We have with us this morning, Mr. H. E. Brown, of Coldwater, Mich., a cement chemist, who is interested in the sand brick industry, and who has taken out patents, I believe, in connection with the same.

Mr. H. E. Brown: I have with me this morning, as you will see on the table, some results of investigation of the sand brick question, as carried out in the laboratory of the Michigan Portland Cement Company, and I will be glad to have you examine them. They were not made under ideal conditions, however, because all had to be done by hand methods, and these methods could not be just exactly the duplicate of those which would be carried on with machinery.

I will say in regard to Mr. Peppel's paper, and concerning cost of installation of a plant, that I think he has been very liberal in his estimates of cost of installation of plants. From some few figures which I have been able to obtain from builders of machines, I think that he has figured very liberally, and a person would have quite a good working capital left, if he had fifty thousand dollars in his pocket with which to build a plant to make 40,000 brick per day under my patents.

You will notice in the tensile tests of brickettes, mentioned in this paper, that they were as high as 600 pounds to the square inch. This is stronger than cement brickettes. Brickettes made of one part of cement and three of sand on a seven-day test, usually give 300 pounds per square inch; on twenty days' test, it usually runs up to 400, 450 or 500 pounds. A brick made from one part of dolomitic lime and nine parts of sand, thirty-six hours after being made, will, when submitted to tensile test, give about a uniform test of 400 to 450 pounds to the square inch. I will be pleased to answer any questions as to the details of the business, as far as I am able. My patents have not as yet been published, and I am now working to secure a better knowledge of the actual working conditions to be met and overcome.

BACTERIAL GROWTH AS A FACTOR IN AGEING CLAY MIXTURES.

BY

EDWARD C. STOVER, TRENTON, N. J.

The ageing of clay mixtures before use, has been practiced in the pottery industry so long, that even the histories of the art do not shed light on when it was first begun. It is quite likely that the benefits in "ageing" mixtures, like numerous other improvements, was first discovered accidentally, centuries ago; perchance, some sanguine clay worker had his fond hopes blasted by his inability to use some new mixture which he had put together with the hope of improving his wares, and after numerous fruitless efforts, was compelled to cast the whole batch aside, and return to his old mixtures. Months, and perhaps years may have elapsed, when a series of conditions arose which prevented him from getting his old supply of materials, and, driven by a pressing demand for his product, he, as a last resort, decided to make another effort with the old abandoned batch, and was astonished to find that it not only worked better and easier, but that he had less loss in drying and firing than ever before in his experience, and thus the discovery may have been made. It has been said that the ancient Chinese potters worked up the mixtures prepared by their forefathers, and and that they in turn put up and stored away the mixtures for the next generation.

Be this true or not, we all know from our own experience, that the proper ageing of clay body mixtures greatly increases plasticity, and lessens cracking in drying and firing, and the questions naturally arise: "What has taken place during this process of ageing, and what has caused this change in the mixture?" The answer to the first is apparent: When a mixture of the different materials is first made, certain affinities or attractions remain unsatisfied, so to speak; they are attracting or in search of other affinities, which, in turn, are also attracted towards them, but owing

to their location in the mixture they are unable to unite, and thus the batch is truly a mixture and not a compound. Proper ageing, however, results in a re-adjustment, as it were, and the searching affinities have found their mates and become satisfied, and the batch has become a compound.

What has caused the change, or made it possible for the mixture to become a compound, is the perplexing question. Having given this subject considerable thought, and after making numerous experiments, I find this change is a fermentation caused by bacterial growth. During the last few years, these infinitesimally small organism have engaged the attention of some of the most eminent scientists, and by their discoveries, particularly in the medical field, our knowledge of the world has been wonderfully modified and enlarged. Quite a number of species of bacteria have been found in water alone, while those found in clays and soils number hundreds.

To get anything like definite data on the subject in hand would require months of constant labor in bacteria culture, and the examination of all of our materials used, and the elimination and classification of the various groups, and the determination of the effects on each of nitrogen, hydrogen and the other common gases and elements generally, and how each class would thrive under different degrees of heat and cold, and how each class would change at different intervals of time, all of which would require more time and attention than could be given by a potter, with his time fully employed each day.

As is well known and established by the works of Pasteur, Koch, Cohn, Tyndall, and many others, fermentation is the result of bacterial growth. Like the investigation of almost any scientific point, that may seem a very small one, and obscure at first glance, this subject on investigation, looms up with innumerable phases, till one inexperienced in bacteriological work is at a loss to know where to begin.

In making examination, I was assisted by I. M. Shepherd, A. M., M. D., a graduate of the University of Pennsylvania, and A. W. Atkinson, A. B., M. D., a graduate of the

Hahnemann College, Philadelphia, Pa., both experienced in bacteriological work, and to both of whom I am indebted for much information and literature on the subject.

The predominating species found in clay mixture is believed to be bacillus sulphureum, also called proteus sulphureus; they give off sulphuretted hydrogen gas (H_2S). Their development is facilitated by a temperature of 37 degrees C, to 38 degrees C, which is equal to 98.6 degrees to 100.4 degrees F.

In a fresh mixture allowed to stand in the slip state, the fermentation will be completed in from six to twelve weeks, according to the temperature and other conditions, without inoculation, but by inoculation, the time is cut down to two to four weeks under the same conditions.

As the ageing of body mixtures is mostly done in damp cellars when clay is fresh from the presses, it could readily be thoroughly mixed and inoculated by pugging and adding at the top of the pug mill a small stream of well inoculated and seasoned slip of the same mixture. Then pack away in the damp cellar, and keep constantly warm and damp, but not moved. In this way the full benefits of a proper ageing could be accomplished in a very short space of time, the many economies of which are obvious.

DISCUSSION.

Mr. Stover: I want to say that Mr. Paul Gates has a microscope and some fine slides in his room, of which you can get some idea from the micro-photographs which I have passed around. Plate A. shows in the center a long snake-like bacterium about ready to break apart. Plate B. shows a number of very small parallelograms clustered together, evidently parts of a large one that has just separated. Plate C. shows a number of the small parallelograms scattered among the irregular dark marks. The former are bacteria, the latter are the particles of clay substance. Mr. Gates extends an invitation to all who may wish, to call at his room and see these little fellows in motion; and I must say that my own ideas were very much less clear until I saw what he has to show. I made several efforts to get some-

thing tangible to bring to this meeting, but did not succeed, and I feel credit is due to Mr. Gates for making this paper so much more real than it would otherwise be.

Mr. F. W. Walker: I am more and more impressed with the great modesty of our members, and those who have had the greatest experience are the most modest. This point touches the white-ware potter particularly. The brickmaker weathers his clay in the open air, under conditions which are not so favorable to the growth of bacteria. We sometimes think our cellars should be kept clean, but Mr. Stover has pointed out that to be useful, they should purposely be kept infected with bacterial growth all the time.

Mr. S. M. Ferguson: The ageing of clays is a big thing in reducing potters' losses. Some white ware potters mix from day to day, and have great losses; while others mix two months ahead, and thus reduce their losses wonderfully.

Mr. D. C. Thomas: Sometime ago our firm began using chemicals to wash our press cloths out. I was rather surprised, and asked why it was done. The reply was that there was something in the sacks that destroyed them. I commenced making inquiries, and learned that a chemist for some southern clay firm had reported that he had found bacteria in the clays. And that the bacteria were what was destroying the sacks. I began then to think that perhaps this question might have some bearing upon the ageing of clays. I had no proof, but I had my own opinion.

Mr. S. M. Ferguson: At one time, we had about twelve presses of slip, which were left in the agitator for about a month, owing to a sudden shut-down. When we started, we pumped it into the presses, and it destroyed a bran new set of sacks entirely. They were no good at all after pumping those twelve presses of slip. The sacks were practically gone. The only way we could account for it was from the ageing of the slip. It was blue when it came from the press, but it should been a white mixture. It was an excellent clay. The hands all said the body had been changed. I said it had not, but they insisted it had, for the clay was now blue, and smelled bad; in fact, the clay was practically

rotten. But in the evening of the first day, every person was telling me the results of the day's work. Every one said that they had better results, less cracking, &c., and the clay worked extra well. But it was hard on the press cloths.

Mr. John Stamm: We are of the same opinion as Mr. Ferguson. We always try to keep the cellar full of clay. The older it is the better, and the less loss we have. That is the way we find it.

Mr. Stover: There is one point I want to make—having been convinced that the little creature is there, we want to use him to the best advantage. We all know that if we get a six month's supply of clay ahead, and there is any mistake, it is a good deal of loss. But if we could accomplish the desired results by adding a well inoculated slip in the pug mill, and thus get the clay in the proper condition to promote the most rapid fermentation, I believe in two or three weeks' time we could get a body which would be as mellow and ripe, so to speak, as we now get in several months' time.

Mr. S. G. Burt: We have had this topic up for discussion sometime ago in the society. The idea, as Seger expresses it, is that as your clay stands in ageing, it gradually developes an acid. I do not think he went so far as to get to the point of bacterial growth. What he clearly claims, and I think proves, is that it is this development of acid which actually brings about plasticity. He gives examples of it by testing clay with a little sodium hydroxide in it, which would not settle for a long time, and when it did, was very weak or tender. He prepared another, I think, with acetic acid, which so increased the toughness of the clay as to make it stand working extra well.

If these animaculæ are going to produce sulphuretted hydrogen, they will have to draw the sulphur from the pyrites present. It does not throw any light on the acid formation. I don't know what organic acid they could create, or whether there is any direct connection between the formation of acid, and the bacteria; but Seger has shown that the formation of an organic acid is the direct cause of increased plasticity in the clay, which is a great benefit to the potter. Mr. Stover and Mr. Ferguson's contributions seem to connect this acid formation with bacterial life.

Mr. W. D. Gates: I hate to differ with my learned brothers on these matters. My impression was that we might cultivate this bacterial growth by changing the chemcal conditions, as much as by changing conditions of temperature—possibly more so. The experiments that we have conducted show that with alkaline conditions the bacterial growth in our slip was greatly increased; with acid conditions, bacterial growth was destroyed.

Another theory of mine is that the plasticity does not so much depend upon the acid condition, as upon a gelatinous secretion made by the bacteria in the slip. It seems to me that a wide study of this would be abundantly interesting. I think the most favorable conditions can be controlled, not only by atmospheric and heat conditions, but also by chemical conditions of acidity or alkalinity; and that by regulating these conditions, we can handle this fermentation much as we please. I think you will all be interested in looking at these microscopic exhibits. They are certainly open to you, and I think will come cheaper to you than they did to me, perhaps.

Mr. Stover: Just a word as to what Mr. Burt said as to acid conditions. Immediately on finding sulphuretted hydrogen, the question arose as to where we got the sulphur. Then came the question, couldn't we get the same results by adding a dilute solution of sulphuric acid. We took some fresh white mixture that had not been inoculated, sterilized it first, so as to be sure there would be nothing alive in it to start with, and added a diluted solution of sulphuric acid with the hopes of doing the same thing quicker. I thought if we could get the result this way in half an hour we could abandon the clay cellar. But I found it would not do it. But a small amount of the dark blue slip-mixture placed in some of this same sterilized material gave the full benefits of ageing in a very short space of time.

THE EQUIPMENT OF A MODERN PAVING BRICK PLANT.

BY

WILLARD D. RICHARDSON, CLEVELAND, O.

While the American Ceramic Society must be concerned chiefly with the higher branches of clay working in which there is a greater scope for science and art, yet, we who are permitted to enjoy membership, even though engaged in the more common and less skillful department of brick-making, expect some light to be shed upon the obscure and uncertain places in our path, some investigations to be made by the society, some facts to be brought out at these meetings that will result in more exact and definite knowledge of the processes we employ.

That noble man, that eminent ceramic technologist, Dr. Herman H. Seger, did not confine his investigations to the problems of the potter, but gave much of his time to the study of the processes of the brickmaker; and the results of his labors, which our society is about to make accessible to the clayworkers of America, are as valuable a contribution to us who are making the common and homely brick as to you who are making the beautiful and artistic pottery wares.

So we brickmakers are greatly benefitted by our connection with this society, though we can give little in return for what we receive. We pay our dues and attend these meetings and listen to the learned discussions of your scientific clayworkers, and absorb and assimilate what we can, and are stimulated to better, more progressive work and perhaps thereby be enabled to enlarge our sphere of action.

For my part, I would not presume to take up the time of this meeting with what I have to offer, but our secretary

has assumed the responsibility of placing me upon the program, and I am pleased to have the honor of presenting a set of plans that I have made for the construction of a plant for the manufacture of sewer and paving brick. The drawings have been made in such detail by architectural draughtsmen that very little explanation is necessary. My experience has taught me that before starting to build, it pays to have every detail carefully drawn out on paper. However well one may have thought out or sketched out his plans, when he comes to assemble the complete details, he will run up against some conflicting arrangements and be obliged to make many changes, as one change generally leads to another. It is much cheaper to make these changes beforehand, on paper, than in wood and iron and brick after the building has been begun.

In planning a brick plant, due consideration must be given to the special purpose of the proposed establishment, and to the special local conditions, such as the size and topography of the site for the building, the location of the clay and the railroad, etc. These local conditions must always determine to a greater or less degree, the general plan to be adopted.

The plans herewith presented are for a paving brick plant, which it is expected will be built in the spring, about one mile south of this city. The shale to be used is situated above the level of the factory, to which it will be conveyed by a gravity railway. The direction from which the shale must come is directly opposite to that from which must come the spur from railroad for receiving coal and shipping brick. These facts necessarily influenced me in settling upon the general arrangement of yard.

I will describe briefly the equipment of the plant shown on the drawings:

The cars of shale enter the factory at an elevation of twenty-two feet and are dumped into hoppers built so that one man, by operating gates from below, can attend to the feeding of three or four drypans, — or the shale can be dumped outside of the hoppers in the storage room and fed to pans, in stormy weather, in the usual manner.

These pans are placed in the pulverizing room back of the press room in a row on a line parallel with the general progressive movement of the clay and brick, so that the clay is easily conveyed from the screens to a central hopper for feeding to pug mills.

The simple stationary inclined screens are used, the clay being elevated from pans to sufficient height to permit of these screens being twenty-four feet long. The width of each screen is six feet, and in order to secure a uniform distribution over the surface of so wide a screen, the clay is dumped from elevators into an U trough conveyor having an adjustable slot in bottom, over the screens. The tailings from the screens pass to the shale hoppers instead of directly to the pans. The screened clay is collected at the foot of the screens by a conveyor that carries it to the hopper above the press room.

The hopper for fine clays is in an upper room separated from the press room by dust-tight partition and floor, the lower part of the hopper projecting through this floor into the press room, and above and between the pug mills. This hopper is also an automatic feeder, supplying, when in operation, a constant uniform quantity of clay to the pug mills, making uniform tempering an easy matter. The capacity of this hopper and feeder is sufficient for the two largest pug mills shown on drawing. These pug mills are placed back to back, at right angles to brick machines, for convenience in receiving and discharging clay.

The brick machines are the standard auger type, largest size, with automatic cutting tables and separating belts.

In this special factory, one machine is to be used for making sewer and building brick, not repressed, and the other for the large size paving block demanded in this market. With the latter machine is shown one double die repress with capacity probably sufficient for this factory, when making these large blocks.

The cars, loaded from the separating belt or repress, pass to a transfer track, just back of the machines at the rear of the press room, and thence to the receiving tracks of the dryer.

The power is transmitted to machinery by a line shaft extending through the center of the building and supported upon cast iron columns with brackets cast upon them.

The boiler room is situated at the lower end of dryer, at the head of the kilns, for convenience in receiving fuel and in furnishing steam for blower engine, and so as to utilize the combustion gases for dryer, if desired, and also exhaust steam. The boilers, of the water-tube type, have induced artificial draft, being connected so as to be operated by separate fan discharging into atmosphere, or by the dryer fan discharging into dryer.

The engine room, between the boiler room and the press room, contains a 250 H. P. Corliss engine for running the factory, and an automatic engine for running generator, dryer fans and machine tools. The machine tools situated in engine room are lathe, planer and drill press. The generator is for running motors that operate fans for induced draft of kilns and also for lighting.

The buildings, you will notice, are practically fire-proof, the walls of brick and the roof framing of steel. This roof frame is covered with matched pine one and three-fourth inches thick and upon this, slate or roofing tile. The underside of the wood, between the rafters, is to be covered with two coats of lime whitewash, or other fire resisting paint. The floors are mostly of concrete and nearly all posts and beams of iron.

The dryer, constructed for the most part in the usual manner, consists of a series of tunnels separated by brick walls — each tunnel containing two tracks 100 feet long. The receiving tracks are twenty-three feet long and the cooling tracks forty-four feet long. The construction of the dryer is absolutely fire-proof, the roof, of course, being the special feature. Upon the top of the division walls, which are six feet and nine inches between centers, is constructed four inches of concrete, made from vulcanite cement, gravel and coal cinders, reinforced with a net work of iron strips. Upon this concrete is a course of hollow book tile laid in cement, and upon the tile is a gravel roof, the felt being stuck to the tile. This roof slopes from the receiving

BRICK MACHINE ROOM

end of the dryer to within ten feet of the discharge end,—one-fourth inch to the foot, this being the slope of the track in the tunnels. The last ten feet of roof at the discharging end is built up to slope backwards, forming a valley. In this valley are openings into 4-inch conductor pipes laid in each wall, and passing downward to the sewer pipe drain below.

The special feature in addition to the roof construction is the slope of one-fourth inch to the foot in the tracks from the receiving end, the last ten feet being level, and the method of moving the cars through the tunnels. The details of this latter are not shown on these plans, but can be briefly described.

The cars are coupled together automatically when placed in the dryer, so as to insure no break in their ranks as they move by gravity down the decline. At the bottom of the decline is a device for braking and stopping the cars. By throwing a lever just outside the entrance of the tunnel, the forward car on any track is disengaged and the car back of it is blocked. After the forward car is removed, the lever is reversed and the other cars let down and blocked, each car being uncoupled automatically about ten feet from the end of the dryer. Thus one car is always free to be removed from dryer, and a car can be put into dryer at receiving end without the necessity of pushing other cars.

The heat for drying is obtained from cooling kilns by means of a fan — the distribution of heat being in the usual manner. The special feature, however, is that the saturated air is removed from the dryer through an underground flue by means of a fan coupled with the fan that furnishes the hot air, both fans being upon the same shaft and driven by the same belt, the driving pulley being in the boiler room upon the shaft extending from the engine room.

In making plans for a brick plant, especially for the manufacture of paving brick, one's chief study must be in designing the kilns for burning. In no other department of brick making, is there greater need of improvement, notwithstanding the large number of patented kilns on the market. Many of these patent kilns possess positive, dis-

tinct points of merit and are well adapted for special situations, but the trouble is that each advertiser recommends his kiln for all purposes and situations. However, I could find no kiln advertised that would meet my requirements for a modern paving brick plant.

The kiln for my purpose must have the following features:

1. Down draft.
2. Uniform distribution of heat.
3. Economical in operation, saving fuel, labor and time, and so constructed as not only to utilize waste heat, from cooling kilns for transmission to dryer, or for heating air for combustion in kiln furnaces, but also it must utilize the waste heat escaping from the hot burning kilns in heating up other kilns. In other words, it must have the advantages of the continuous kiln, without its disadvantages.
4. Durability — costing little for repairs.

The first question to be determined was whether to have separate kilns, each of which could be burned independently, if desired, or to have the several ovens or chambers so placed in juxtaposition as to preclude one being burned independently of the others. I was influenced somewhat in this by the judgment of friends who have had large experience with continuous kilns, and also by the fact that in building separate kilns, I would be following along the line of my previous practice, and would feel more confident of such results as I wish to obtain. I do believe, however, that a continuous kiln can be built that will be highly satisfactory for paving brick.

Having decided upon separate kilns, the next point to determine was the form of the kiln — whether round or rectangular. My experience with both forms enabled me to decide at once in favor of the round kiln, chiefly for two reasons:

1. Because the round kiln is less expensive in first cost and especially in repairs.
2. Better distribution of heat and fewer underburned brick.

The stack question came up next for consideration — whether one or more stacks, and where situated. You will notice that I decided upon the single stack in center of kiln. The advantage of this arrangement are:

1. High temperature of stack, hence a low stack.
2. Heating up the stack from the first firing means a strong draft in the early part of burning, giving freedom from whitewash and shortening of time of burning.
3. A kiln of larger diameter with hard brick to the center.

The disadvantage of stack in center of kiln are:

1. Inconvenience in setting.
2. Space occupied in kiln.

These disadvantages I have found not so great as I first thought. The stack in the center of a large kiln is not much in the way — in fact, hardly more in the way than a stack in any other place. The space occupied in the kiln is for a thirty-foot kiln only about two per cent. of the inside kiln space and for a thirty-six-foot kiln, about one and one-half per cent.

The special feature of the kilns for this plant is the system of utilizing the hot gases escaping from the burning kilns for heating up other kilns, and likewise of using the cooling kilns for heating the air for combustion. To accomplish this the kilns are connected by an underground flue, extending from the center of each kiln to a circular distributing flue in the next kiln ahead. This circular flue is in the hub of the kiln, with openings into each fire box, adjustable by dampers. The movement of the gases is induced by a fan connected to the stack of any kiln by a cylindrical steel flue, as shown in drawing. The fan is portable, being erected upon a car moved along an elevated track between two rows of kilns and operated by an electric motor directly connected.

I calculate in this manner to be able to draw the heat from a hot kiln through at least two other kilns and that by the time that the kiln under fire is finished, the kiln just ahead will be partially or wholly incandescent, so that heavy fires can be put into the furnace at once. There will thus

not only be a saving in fuel, but a considerable saving also in the time of burning.

Whether I accomplish all that I expect by this system or not, I am confident that I can accomplish enough to more than pay for the additional expense of construction. However, each kiln can be burned independently by natural draft, if desired.

Now, gentlemen of the American Ceramic Society, I have given you the result of my thought and experience upon the subject assigned me. I have no interest in influencing anyone to adopt any of the suggestions I have made. I have tried to do my duty in presenting this paper, and I hope to have the benefit of your criticisms.

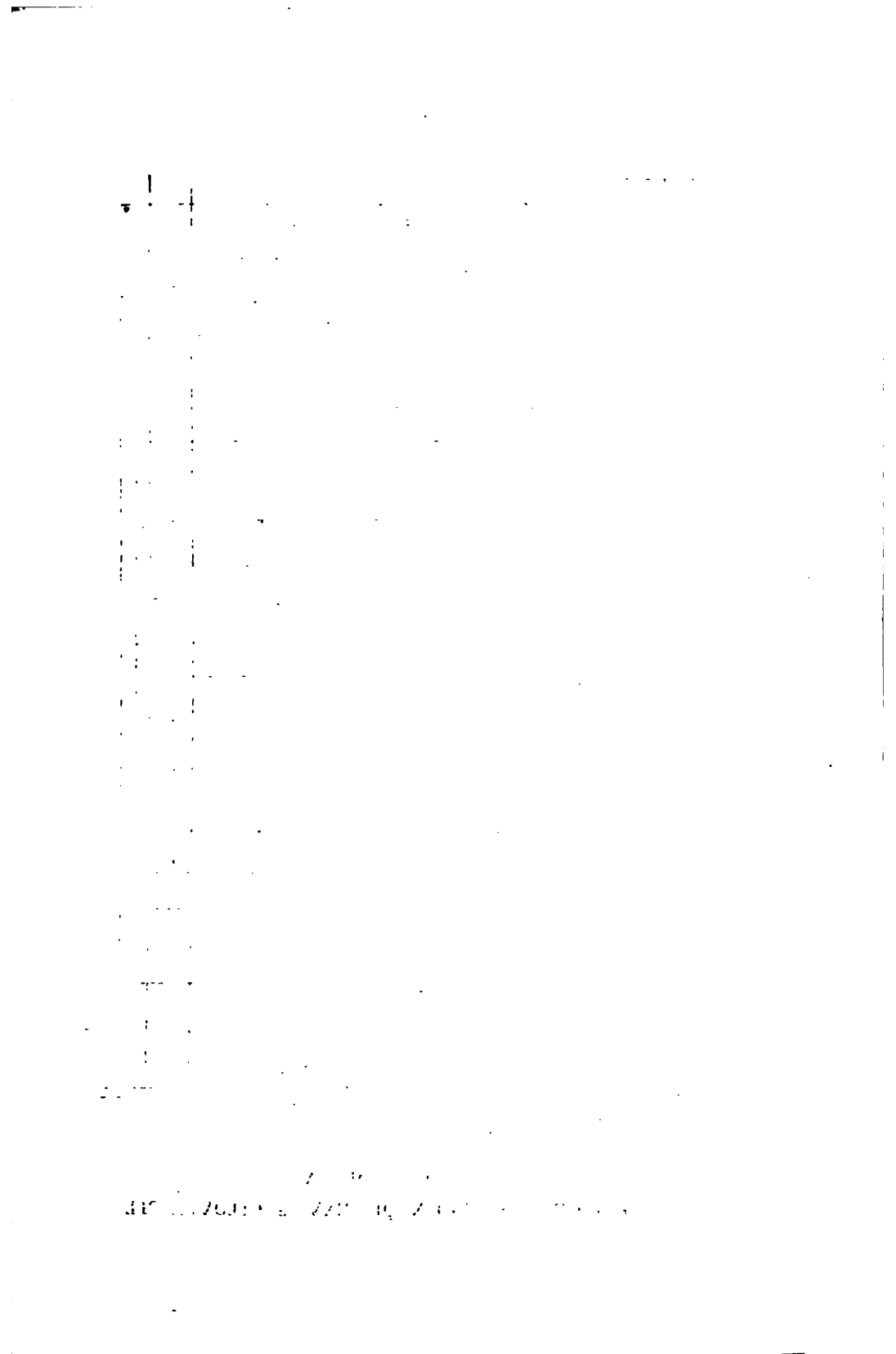
DISCUSSION.

Mr. Lemon Parker: How much grate surface do you provide as compared with the size of the kiln?

Mr. Richardson: I have not gone into many of the details, because I did not know whether it would be of sufficient interest to those who would listen to it. There is really no grate surface at all. These are thirty-six foot kilns, with twelve furnaces to each kiln. The fire-hole is two feet wide, without grate bars. As shown in the plans, the firing door is covered in the usual manner with a fire clay slab, reinforced with iron bands. The coal is thrown upon a coking table, made partly of cast iron and partly of fire-clay tile and shoved down upon the fire just before a fresh supply of coal is added. A furnace, similar to this, is used quite a good deal in Ohio, but this drawing here is not exactly, perhaps, as I would make it for this kiln. Since this drawing was made, I have built furnaces which I think are better than these. But they are built on the same principle—down-draft furnaces, with a coking table and no grate bars.

Mr. Parker: We use fourteen fire boxes for a twenty-five foot kiln.

Mr. Richardson: Many people will not use as many fire boxes as I have shown here. I never built a kiln larger than thirty feet; on that, I used ten fire boxes, and had no trouble in getting plenty of heat in the kiln.



Mr. Parker: What cone did you burn to?

Mr. Richardson: In the kilns I referred to, we burned iron mottled, fire-flashed brick, cone eight down, on the bottom.

Mr. Parker: We burn cone ten.

Mr. Richardson: That is a higher temperature than is usually required in the brick business. Still there would be no difficulty in getting that temperature. But probably if you require that temperature, it would be well to have wider furnaces, or more of them.

Mr. J. Parker B. Fiske: I have been exceedingly interested in this paper and in the drawings, not only on account of their technical value, but because it seems to me this paper marks an era in the brick manufacturing world. We have had presented here a carefully prepared set of plans for a brick plant, based on an experience of many years in brick manufacture, and worked out by a competent engineer. The mechanical engineer up to the present time has not applied himself very largely to the designing of complete clay-working establishments, particularly those for manufacturing brick.

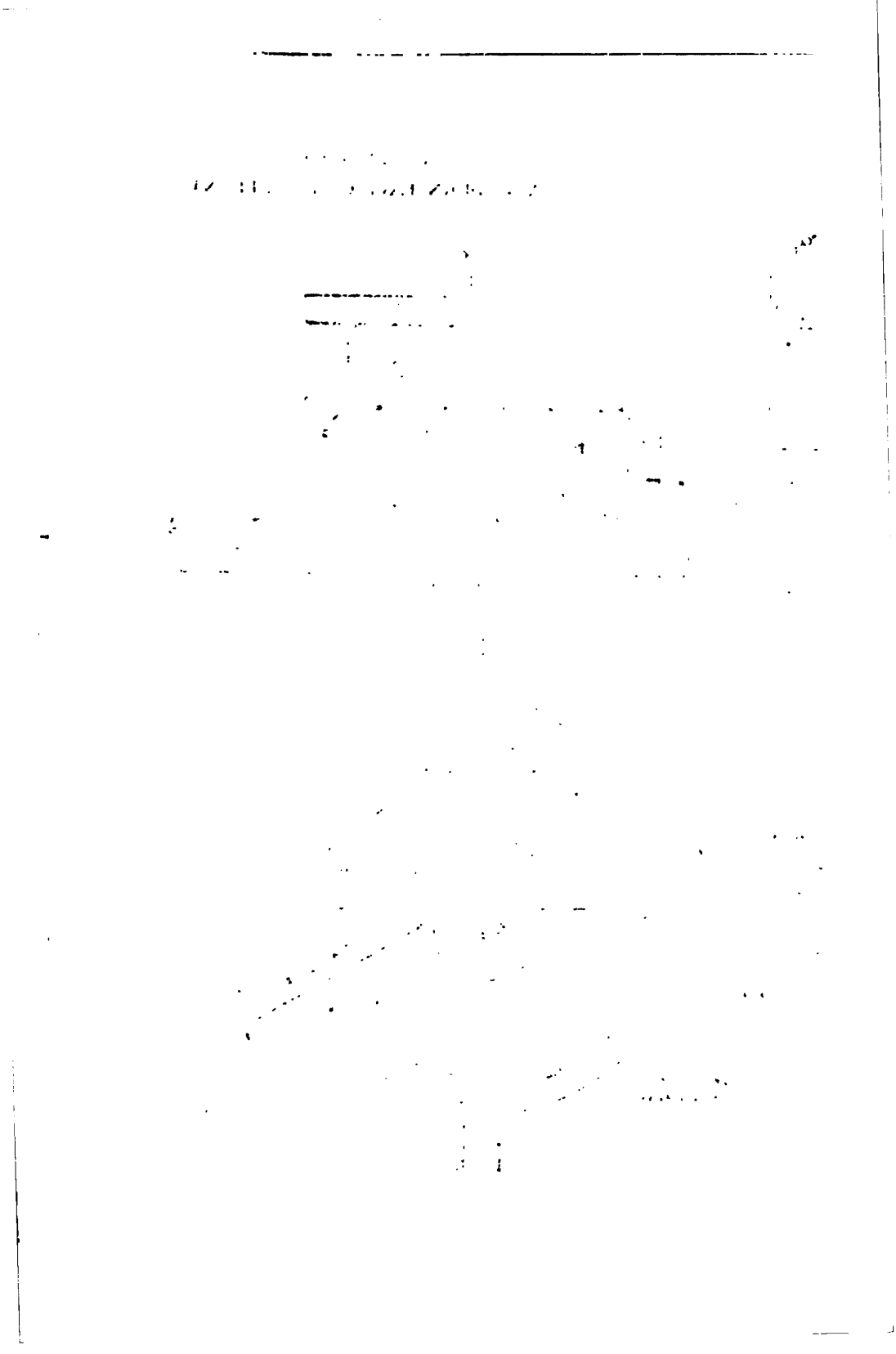
The manufacturer, as a rule, has had to gather such points as he could from his neighbors, coupling with these his own experience, and then with a very meagre set of plans (if any at all), has had to commence the construction of his plant. And I venture to say that the average brick plant, which, like Topsy, just grows, without any predetermined plan, is hardly ever well commenced before the builder finds that some things are wrong, some parts don't work together to advantage, and he wishes he had built differently.

Now, the building of any large manufacturing establishment today, outside of brick-making, is rarely ever undertaken without the most careful consideration of every feature entering into the construction and operation of the plant. A modern steel or cotton plant is designed by the most competent engineers before anything else is done. I believe that the losses which the brick manufacturer suffers today, more or less in every plant, are largely due to the

fact that the whole arrangement is not carefully considered on paper, before building is attempted. Now, of course, no matter how carefully a plant is worked out on paper, there will be some things discovered during the process of construction on which should be altered; but with a careful consideration in advance of every feature of the complete plant, as in this case, from the arrival of the raw clay to the finished product, there cannot fail to result economy of labor, economy of fuel, and cost of maintenance far ahead of that in a plant otherwise erected.

There are many points about this plan in which I am interested, and which I would like to discuss with Mr. Richardson, but I will not take the time of the society for that. I would like to ask one or two questions, however: One is, what particular advantage Mr. Richardson sees in the arrangement of isolated kilns, coupled together for continuous operation, over the construction of a kiln where the chambers are all built in one structure and operated continuously?

Mr. Richardson: Well, as I stated in my paper, I do believe that a continuous kiln can be specially designed to burn successfully any kind of product. But, in making plans for a plant in which you are to put your own capital and that of your friends, you feel that you dare not be too radical for fear that something might be wrong somewhere. I have, in fact, gone all over that question. I have been first on one side and then on the other, and I have tried to include the advantages of both systems as far as possible. The advantage of this plan is that any kiln can be burned separately, independent of the rest of the plant whenever it is desired to reduce the output or to make repairs; and also, that for paving bricks, probably better results can be obtained. The vitrification and proper annealing of paving brick no doubt can be done in a continuous kiln, but there were too many questions in my case as to just how it was best to do it, and whether it would be a decided success. I know the continuous kiln is being used for paving brick in a few places, but to the best of my information, not



as successfully as the down-draft kiln. It may be owing to the fact that the continuous kiln is not properly designed for this special purpose. I am a little inclined, myself, to go wholly into the continuous kiln line, but have been afraid to take the risk in such a plant as I have here designed and where all depends on the efficiency of the single kiln system.

LEAD FRITTS AND THEIR ADAPTATION TO POTTERY GLAZES.

BY

WILTON P. RIX, NEW CASTLE, ENGLAND.

The attention of the English potters has of late been directed to the relative value of lead in a raw and fritted state, by the proposal to enforce regulations forbidding the use of raw lead in the manufacture.

Statistics support the statement made that the use of fritted lead reduces the noxious quality of the glaze, but it has been shown, beyond doubt, that in order to obtain complete freedom from plumbism, it is necessary that the lead contained in the fritts used should be insoluble in dilute hydro-chloric acids; that is, that the lead should be present only in the form of bisilicate or compound silicates complying with the above tests.

This matter has really assumed still greater importance, owing to the comparative failure of the attempts made by a number of the English earthenware manufactures to obtain satisfactory leadless glazes, capable of being worked under the ordinary conditions. The average loss has been found considerably greater with the use of such glazes than in the case of those containing lead, owing to the greater care required in the manipulation and firing such glazes.

In attempting the substitution of fritted lead glazes for those compounded with raw lead, various important considerations arise with regard to their adoption. These relate to apparent disadvantages which has been raised against them; also to the possibility of adjusting the fritts to the present requirements of the various branches of the trade.

The objections may be summed up as follows:

First. The additional cost involved in fritting as regards fuel, wear and tear and labor.

Second. The loss of lead by evaporation and absorption in kiln fritting.

Third. The difficulty of producing insoluble fritts of sufficient fluidity to allow of their introduction into the softer glazes, without a sacrifice of the brilliancy and soundness of the glaze.

Fourth. The increase of the difficulty of manipulating and firing such glazes.

With the adjustments of such fritts to the present glazes and conditions, the question at issue may be stated as follows :

The continental potters appear to have realized that the use of fritted lead assists the immunity of the lead worker from plumbism; indeed many of them have adopted fritting with promising results, while a few of the most important works have produced glazes from fritts of very low solubility.

The temperatures at which such continental earthenware glazes are matured vary from 06 to 01. The temperature for maturing English earthenware glazes varies from code 05 to cone 03. The percentage of lead used in English earthenware glazes varies from 18 per cent. to 25 per cent. The percentage of lead used in English china glazes varies from 3 per cent. to 20 per cent. The percentage of lead used in English majolica glazes is from 35 per cent. to 55 per cent., or even 60 per cent. The temperature for maturing majolica glazes varies from cone 010 to cone 05.

These being the existing conditions the questions arising are:

First. Does the substitution of lead fritts as in raw lead involves any alteration in actual properties of the glaze components in order to obtain results similar to those of raw lead glazes?

Second. Does the use of fritted lead involve any increase of firing temperature, or any different method of carrying on the firing process?

Third. Is the fluidity of the glaze affected by the character of the lead fritts used; that is whether bisilicate or double silicate or compound silicate is used?

Fourth. Can glazes of sufficiently high fusibility be obtained for the manufacture of plumbiferous majolica

glazes, fired at Seger cone 010, when these are compounded of lead bisilicates or double silicates, the maximum solubility of the glaze not exceeding, say 3 per cent.?

Fifth. How far is it possible to introduce other fluxes into the fritt mixtures, thus increasing fusibility and yet still maintaining the requisite insolubility of the lead present: and having also due regard at the same time to the effect in use of such fritts?

(*a*) On the working properties of the glaze in firing.
(*b*) On the tints of colored majolica glazes.
(*c*) On the fluidity of the glazes when fired at the customary temperatures.

On many of these points the English manufacturers appear at present to be at variance. Some imagine that the use of lead fritts cannot be carried out: while others show satisfactory results and have no objection to raise as to their adoption. Probably this discrepancy arises from varying conditions coupled with a want of efficient manipulation, and also insufficient attention to details in the case of some who have failed.

It is obvious as regards the use of lead fritts in the glaze that the lower the firing temperature, the greater the amount of lead required. When this amount exceeds that present in a bisilicate fritt, new difficulities arise in the production of a double or compound silicate, capable of holding in combination such fluxes other than lead as will affect the necessary fusibility of the fritts and consequently of the glazes.

It will be seen that various other existing questions are involved which will readily suggest themselves to members of the society, and on which at present little accurate information exists, but those mentioned above are more than sufficient to occupy the scope of the present paper. I therefore propose to limit the discussion to the points first named.

(1) *The possibility of substituting lead in fritted form for raw lead without change of proportions used.*

On this point my experience does not bear out the statement made by some manufacturers, that more lead is

required to produce the same results if introduced in the form of fritt. I have made comparison of the same glazes worked under both methods side by side in the same saggers, and have repeated these in different ovens. The results do not vary in any substantial respect provided that the total contents of the glaze remains the same, and the calculations of the new formulæ are accurately carried out—the raw and fritted glazes for the lead may be substituted for raw lead.

(2) *Does the use of fritted lead involve any increase of firing temperature?*

Provided the total contents of the composition of the raw and fritted glazes are the same, the raw and fritted glazes will be found to fuse at the same temperature. There is, however, some indication that some lead fritts take longer to mature in firing than others. The point of incipient fusion must also be placed somewhat higher as regards temperature. There is evidence of this fact in composition of fritted glazes now in use on the continent by some important manufacturers, who add a very small percentage of raw lead to the glazes, obviously for the purpose of promoting the incipient fusion at a lower temperature. I attribute to these questions of incipient fusion and length of firing, the difference of results obtained by various potters. It is probable that the firing is carried on by different methods, or that the uniformity of heat in various positions in the oven is less with some than with others.

(3) *Is the fluidity of a fritted lead glaze affected by the character of the lead fritt used; that is whether bisilicate, double silicate or compound silicate.*

On this point there appears to be strong evidence that the fluidity is thus affected: that is, apart from the question of fusibility the glazes when fused are more or less viscous, according to the composition of the fritt. It is, however, open to further research to demonstrate how far this fluidity of the glaze at a good temperature if affected by the heat at which incipient fusion takes place, and also by the length of time required to mature the glaze in firing, without increasing the temperature. This issue is one of considerable importance to the potter in adopting fritted glazes for use on

goods printed under glaze. My own experience over a limited area is that double silicates of lead show less fluidity than bisilicates if worked under the same conditions, the equivalents being of course duly regarded in the fritt composition.

(4) *Can glaze of sufficiently high fusibility be obtained for majolica colors, firing at cone* 010 *by the use of bisilicates or double silicates of lead, still maintaining a solubility not exceeding, say three per cent.*

Before discussing this question it is necessary to define the amount of fluidity required in the glaze. For plain tiles, needing only uniformity of color and texture, the property of extreme fluidity and transparency is often less important than in the case of finely modelled relief tiles and blended glazes, in which the artistic effect is mainly due to the falling of the glaze into the deep hollows, and the ready mingling of the colors with each other during the firing.

In the latter case considerable difficulties arise in producing suitable glazes with bisilicate lead fritts. Many glazes now in use for this purpose contain as much as fifty to sixty per cent of lead oxide, and it is obvious that even supposing the bisilicate fritt by careful preparation is obtained under two per cent. solubility, the high percentage of lead contents required in such glazes leaves no room whatever for the introduction of other glaze materials to improve its working properties.

It must also be pointed out that the method adopted of obtaining some colors by the dilution of a boro-silicate colored lead fritt with transparent lead glaze, is no longer available when the two per cent. solubility is exceeded, and even if the raw lead is replaced by lead fritt. The high solubility of such boro-silicates forbids their use in these conditions, such as the difficulty of obtaining low solubility with sufficiently high lead contents in the glaze to insure the extreme fluidity on which the beauty and richness of many soft majolica glazes depend. In this branch it appears to me there is ample scope for further research, and it is probable that eventually the introduction of a new pallette of

colors may be involved — or a new type of decoration less dependent upon the property extreme fluidity.

In discussing the above questions it must be understood that the production of glazes having a maximum solubility of two per cent. has been kept in view, and also the maintenance of existing conditions in English pottery manufacture as to composition of glazes and firing temperatures, and adaptation to existing bodies.

The difficulties are obviously much diminished as the firing temperature is increased, because the lead contents of the glazes can then be proportionately dimished.

DISCUSSION.

Professor Charles F. Binns: This lead question has been an interesting study, and has cost English manufacturers a good deal of heart-burning and anxiety. One important point has been brought forward in the relative solubility of lead fritts. It is found that lead fritts are more soluble with boracic acid than without. This was proved out by a Belgian manufacturer. I doubt if this fact will affect the bulk of small manufacturers in England, for they are troubled enough in making one fritt, without making two. It is scarcely known here how far the practice exists among the smaller English manufacturers of buying ready-prepared glazes. Quite a number buy it ready prepared and have no more idea what is in it than the man in the moon. This operates against anything like a change in glaze. As the matter stands now, the English manufacturers have been given eighteen months in which to make experiments, and my view is that the burden has been materially lightened as to the potter.

I want to say a word about Mr. Rix's last remark regarding the expansion of the ceramic society. The North Staffordshire Ceramic Society has been established since we have been in existence. How far they have thrown their net around the manufacturer, I do not know. They meet once a month for the discussion of minor problems; and I have wished sometimes that we could meet once a month and have one thing at a time. We are in great danger of ceramic dyspepsia from the full meal we have had here.

One more point. I wish Mr. Rix had defined his bi-silicate and double silicate and compound silicate. We ought to have further information on this point.

Mr. E. E. Gorton: My experience differs somewhat from Mr. Rix's on one point, in regard to the fritting of the lead. I have found that where lead is fritted, and the glaze is not a boracic-acid glaze, it does require a higher equivalent of lead to get the same results. I presume it is due to the volatilization of lead in fritting.

Professor Binns: I ought to make another remark. This question of the solubility of the fritt created quite a good deal of disturbance. The English regulations required the inspector to take samples from the dipping tub, and if he found more than two per cent. of raw lead the manufacturer could be brought to account. The point has arisen as to what constitutes solubility. Mr. Jackson carried out a line of experiments and found that solubility was directly commensurate with the size of grain. By repeated grindings, he found he could dissolve more and more as the fritt became finer, and it seemed that if he could go on long enough he could dissolve the whole. His conclusion was that a coating of insoluble lead was formed on the grains of fritt, and that the grinding removed this each time.

Professor Orton: Wouldn't they be driven to construe the question as to the amount of lead in solution in the dipping tub?

Professor Binns: Yes — not in solution in the dipping tub, but in suspension. The per cent. of raw lead in the dipping tub is one thing and the solubility of lead fritt is another. The government said, "You must not use more than a certain per cent. of raw lead." The manufacturer laid, "We can't make glazes with that amount." Then the question arose, could not the lead be rendered insoluble. It was suggested that by fritting it might be rendered insoluble. They can use raw lead in the dipping tub, providing it is not above a certain amount.

Professor Orton: Does English legislation forbid the use of a glaze in which more than two per cent. of lead is soluble?

Professor Binns: That is what was proposed. We were obliged to provide overalls, caps, nail brushes, soap, towels, etc., in order that the men and women should be compelled to wash themselves, and clean their nails. The whole trade is in revolt owing to this ill-judged and "grand-mother" legislation, from which we in the United States glory that we are free. The next point which arose was in reference to a series of rules embodying the limits of raw lead which could be used. The investigation which was made has entirely broken down on the government side, and it has nothing to say at present, though it may have eighteen months hence. It has been demonstrated that if the regulations are enforced the whole pottery industry of England will be killed.

Mr. Burt: I always understood that the poisoning, as a rule, was due to the inhaling of lead dust. I don't see any direct connection between lead dust and the question of solubility, whether two or three per cent. is in solution.

Professor Binns: The issue is somewhat simple. It is due to the effect of the juices of the body, particularly the gastric juice upon the lead oxide; and the effect of dilute hydrochloric acid is considered to be the same as that of the gastric juice. The point was whether lead oxide, if soluble in hydrochloric acid, might not be absorbed by the skin. Not only might this be so, but sometimes the work-girls would carry candy in their pockets and feed themselves while their hands were covered with lead. It gets into their hair, and the body pores can take in a vast amount of the lead salts. The effects are shown to vary with different persons. I have known a case where a girl was put to work trimming a porcelain glaze not containing more than four per cent, of lead oxide, and she sickened in a week. And there are cases where women have worked twenty years and not felt the slightest ill effect. It is largely a matter of tempermanent, and we always made it a rule when anyone showed any signs of sickness to put them elsewhere, only keeping in that department those who showed themselves to be immune.

THE STRUCTURE OF SILICATE MIXTURES.

BY

HARRISON E. ASHLEY, S. B., NEW BEDFORD, MASS.

"Based upon the microscopic examination of thin sections of minerals and rocks, observations were recorded in 1858 by Dr. H. C. Sorby, member of the Iron and Steel Institute, in a paper on the microscopic structure of crystals, indicating the origin of minerals and rocks (Quarterly Journal of the Geological Society, vol. XIV, page 453), and in October, 1867, by the late Mr. David Forbes, member of council and foreign secretary of the Iron and Steel Institute. These observations gave birth to the special science of petrography." *

"From the introduction of microscopic methods of investigation up to a date about eight or ten years ago, the energies of practically all students of the subject were devoted to observing and recording mineralogical and textural differences, and the subject of chemical composition received but slight attention. It was revived, however, toward the close of the eighties by W. C. Brögger, then in Stockholm, and in the course of time has received wide recognition and employment from many others.

Petrographers are now accustomed to recast an ordinary chemical analysis by dividing the several percentages by the molecular weights of the corresponding molecules, so as to obtain a series of numbers, which are called the 'molecular proportions' or 'molecular ratios.' These quantities indicate the relative numbers of the several molecules in the rock magma, and in that respect are more significant than are the percentages. . . . . The molecular composition of the common rock-making minerals are now quite accurately determined and understood, and using them

* The Metallographist, (1902), 5 [3], 146.

it is often possible to calculate from the molecular proportions furnished by a rock analysis, the percentages of the several minerals in the rock. The calculations are usually checked in a general way by a study of thin sections." *

In a recent joint report by W. H. Weed and L. V. Pirsson, † the latter presents recalculated analyses of a large number of igneous rocks. He first ascertained by microscopic examination what minerals were present; then from chemical analysis and molecular proportions what percentage of the several minerals were present. Prof. J. F. Kemp, the author of the article on 'The Re-calculation of the Chemical Analysis of Rocks,' from which the two preceding paragraphs were copied, shows in detail in that article the method of recalculation.

The present writer, in an article § on slag-constitution, has shown that the freezing or melting points, or less exactly, the formation temperature, or the total heats of solidfication will show the constitution of such mixtures at the temperature of fusion.

Chemical methods of separation, taken by themselves, have not been very successful in showing the structural constituents of silicate mixtures.

It is most satisfactory to rely, not on a single method of investigation, but on the joint use of several methods. It is desirable to supplement chemical analysis and fusibility determinations by microscopic examination. The latter often presents special difficulties. The slag or ware may be unsuited for the preparation of thin sections, or if suited, few persons have the necessary equipment for their preparation, or considerations of time and expense may be prohibitory. Silicate mixtures are seldom examined on the surface of thick pieces by reflected light, as it is difficult to find any apparent differences of structure. With metals and alloys on the other hand, surface examination by reflected light is

* School of Mines Quarterly, (1900), 22, [1], 76.

† Geology of the Little Belt Mountains, Montana, by W. H. Weed, with a report on the Petrography by L. V. Pirsson, XX. Ann. Rep. Dir. U. S. Geol. Survey, III., 257.

§ Slag-Constitution, studied by means of the Tri-Axial Diagram, with Rectangular Co-ordinates.—Trans. Am. Inst. Min. Engineers. Nov. 1901.

the universal method. Differences in structure are shown by the unequal action of polishing agents, of etching agents, and of oxidation, or other coloring effects. With silicate mixtures, so far as the writer knows, nothing has ever been accomplished by differences in wear or by development of colors, and but little by etching with fluorides or alkalies.

A phenomenon met in laboratories may offer a means of progress. The writer has frequently noticed that the first portion of vapor condensing on a cold glass surface, for example, the underside of a watch-glass upon a beaker whose contents are being heated, is not uniformly distributed over the whole surface, but is in small patches. Each of these patches seems to be uniformly covered, and differs in density from the patches touching all around it. The whole effect resembles the microstructure of a slowly cooled very crystalline metal. Each patch of moisture may indicate the extent of the external surface of one crystal* of the glass. With further condensation the differences are obliterated.

It seems likely that the first microscopic deposit may be more highly differentiated than that that appears to the naked eye, more destinctive in character, and more certain of positive appearance. While water vapor is a fugitive medium, there are multitudes of definite chemical substances capable of being sublimed with the formation of fairly stable deposits.

It is possible that the selective action of the silicate mixture crystals may even go so far as to attract or retain, substances having the same crystal system and molecular volume, while rejecting substances of unlike properties. This may open a way for the study of molecular weights of solid substances.

The writer regrets that he is not at present in a position to test and develop the method of research here proposed.

*H. M. Howe: "What is the essence of crystalhood?" Metallographist, (1902), 5, [1], 52.

THE RELATION BETWEEN THE CONSTITUTION OF A CLAY, AND ITS ABILITY TO TAKE A GOOD SALT GLAZE.

BY

LAWRENCE E. BARRINGER, E. M., SCHENECTADY, N. Y.

The subject of salt-glazing has received but little attention from investigators in ceramic work since its introduction by Flemish potters in the thirteenth century. Considering the extent to which the process is employed in the manufacture of clay products in this and other countries, and the fact that but little is known of the reactions of the process, and still less as to what materials are most suitable for such treatment, it is a matter of surprise that no attempts have been made to throw some light on this important branch of clay glazing.

In English ceramic literature the subject has received little or no consideration. The German writers have given some attention to the subject, but the bulk of the articles are either historical or deal with the actual practice in accomplishing the desired results.

The only article of real investigation which the writer has been able to find, is that on the action of iron in clay during salt-glazing, by Jos. Knett, published in Thonindustrie-Zeitung, 1896. Knett's experiments, while leading to highly interesting results, do not attack the truly fundamental principles of the process.

A desire to at least awaken a little interest, if possible, in original research along the line of salt-glazing, led the writer to make the investigations noted in this paper.

It is a well-known fact that some clays salt-glaze better than others, while some are not at all able to be glazed in this manner. Furthermore, the character of the glaze is different on different clays, as regards color, hardness and finish.

In order to draw a line if possible between salt-glazing and non-salt-glazing clays, it was decided to attack the subject by analysing both the clay and glaze from a factory where the work is successfully carried on, and from this basis vary the composition of the clay until its limit of ability to take a good glaze was reached. To this end, the clay used at Logan, Ohio, by the Hocking Clay Manufacturing Company, was selected. The salt-glazing done by this concern is of a high order, and their products are first-class representatives of salt-glazed clay wares. The process is here applied to bricks, sidewalk tiles, sewer pipe, garden urns, etc. The clay used is a fire clay of the Lower Kittanning horizon, the vein being pretty well weathered.

The methods used in securing samples of the clay and glaze for analysis were as follows:

SAMPLING THE BODY AND GLAZE.

In order to obtain a fair average sample of the clay as it entered into the product, a barrel was placed near the chute leading from the bin containing the screened clay from the dry pan, into the wet pan. Into this barrel the wet pan attendant threw a little of the ground screened clay at intervals, filling the barrel at the rate of about one-fifth each day. In this manner a fair average sample of the clay used in a five day's run was obtained. The contents of the barrel were then dumped onto a clean cement floor and thoroughly mixed, finally quartering down systematically until about thirty pounds remained. This was placed in an iron ball mill with flint pebbles, and ground fine. The finely ground clay was then spread on a table and mixed thoroughly, and then quartered down until a sample of a few hundred grams was obtained, which was bottled and labelled, and constituted the sample from which the analyses were made.

The glaze sample was obtained by chipping the salt-glaze from bricks taken at random from different stock piles, the only consideration being that the glaze should be sufficiently thick to chip off nicely. Bricks on which the glaze had run more or less towards one edge were chosen, and the glaze was carefully chipped off along this edge with a small cold

chisel and light hammer. About twenty grams of the glaze was secured in this way. From this, the sample for analysis was secured by selecting pieces of clear glass from the latter, rejecting any chips which showed any indication of adhering body. The chips of glaze thus selected were ground fine in an agate mortar, and weighed out for analysis.

ANALYSIS OF THE BODY AND GLAZE.

Two carefully made analyses of the clay gave the following results:

	No. 1	No. 2	Average
SiO_2	63.45	62.77	63.110
Al_2O_3	23.04	23.56	23.300
Fe_2O_3	2.08	2.44	2.285
CaO	.72	.78	.725
MgO	1.08	.91	.970
Na_2O	.58	.45	.490
K_2O	.98	.93	.930
SO_3	.24	.24	.240
H_2O	7.81	7.81	7.810
	99.78	99.84	99.810

Two analyses of the glaze gave:

	(a)	(b)	Average
SiO_2	55.60	55.35	55.475
Al_2O_3	21.14	21.54	21.340
Fe_2O_3	2.54	2.74	2.640
CaO	3.55	3.45	3.500
MgO	.04	.04	.040
Na_2O	16.91	17.51	17.210
K_2O	.08	.08	.080
	99.86	100.71	100.285

The glaze gives the following formula:

$$\left.\begin{matrix}.812\ Na_2O\\ .002\ K_2O\\ .182\ CaO\\ .002\ MgO\end{matrix}\right\}\ .998\ RO \quad \left.\begin{matrix}.612\ Al_2O_3\\ .048\ Fe_2O_3\end{matrix}\right\}\ 2.704\ SiO_2$$

The clay reduced to a formula becomes:

$$\left.\begin{matrix}.056\ CaO\\ .105\ MgO\\ .084\ Na_2O\\ .042\ K_2O\end{matrix}\right\}\ 1.00\ RO \quad \left.\begin{matrix}1.00\ Al_2O_3\\ .06\ Fe_2O_3\end{matrix}\right\}\ 4.605\ SiO_2\ (+1.957\ H_2O)$$

The glaze reduced to the same type of formula as the clay, viz., $Al_2O_3 = 1$, enables the following comparison to be made:

$$\text{(Clay)} \quad \left.\begin{matrix} .056\ CaO \\ .105\ MgO \\ .034\ Na_2O \\ .042\ K_2O \end{matrix}\right\} 1.00\ Al_2O_3 \left\{\begin{matrix} \\ .06\ Fe_2O_3 \end{matrix}\right. \left\{ 4.605\ SiO_2 \right.$$

$$\text{(Glaze)} \quad \left.\begin{matrix} 1.326\ Na_2O \\ .003\ K_2O \\ .297\ CaO \\ .008\ MgO \end{matrix}\right\} 1.00\ Al_2O_3 \left\{\begin{matrix} \\ .078\ Fe_2O_3 \end{matrix}\right. \left\{ 4.418\ SiO_2 \right.$$

It is seen that the silica is a little higher in the body than in the glaze, the alumina being the same. This is not to be regarded as strange, as the salt fumes cannot be expected to attack all the constituents of the clay with equal ease and incorporate them into a glass in exactly the same proportions as they exist in the clay. A certain per cent in the clay of coarse free sand which the salt does not easily attack, would no doubt lower the silica content of the glaze below that of the clay. Or, an aluminous coal-ash would tend to increase the proportion of Al_2O_3 to SiO_2 in the glaze over that in the clay, by being carried up into the kiln with salt vapors and lodging in the glaze.

A noticeable and interesting difference is in the amount of lime in body and glaze. While the clay shows only .725 percent of lime, the glaze shows 3.5 per cent. The most reasonable explanation for this difference seems to be that of efflorescence. The fact that this clay contains soluble sulphates, and is, moreover, known to efflorescence, might easily explain the presence of so much lime in the glaze. The soluble salts were brought to the surface and left there as a coating, long before the salt was applied to the fires. On salting the kiln, this efflorescence (mostly $CaSO_4$) was naturally absorbed as a part of the glaze.

While it is seen that the ratios of alumina to silica in clay and glaze are not exactly the same, still they are very close. From this we may reasonably infer that the ratio of alumina to silica in any clay which can be salt-glazed, is a good index of approximately what this ratio will be in the salt-glaze itself.

Langenbeck* holds that the molecular ratio of alumina to silica in a salt-glaze should be about 1 to 8, and assuming that a clay to be properly salt-glazed should contain alumina and silica in a similar ratio, he states that a clay best suited for salt-glazing will contain its alumina and silica in a molecular ratio of about 1 to 8.

This ratio of Langenbeck's is not borne out by the above analyses, nor by subsequent actual tests made by the writer. While it was found that a clay containing alumina and silica in the ratio of 1 to 8 would salt-glaze well, it was also found that the ratio could be much lower or much higher, and an equally good glaze could be obtained.

THE EXPERIMENTS PROJECTED.

To attain the object of this paper, namely, to throw some light on the relation between the constituents of a clay and its ability to be successfully salt-glazed, two series of experiments were first taken up, one decreasing the ratio of alumina to silica over that shown by the clay analysis, by additions of sand; the other increasing the ratio of alumina to silica, by additions of kaolin.

Starting with the Logan clay unadulterated, systematic additions of sand were made until a ratio of alumina to silica of 1 to 10 had been reached. This, it was thought, would show a point where the silica would be too high for a good salt-glaze.

This series is shown by the following table, in which the composition of the various mixtures proposed is indicated by their molecular formulæ:

SERIES I.

No.	KNaO	CaMgO	Al_2O_3	Fe_2O_3	SiO_2	Ratio Al_2O_3 to SiO_2	Remarks
8	.076	.161	1.00	0.06	4.60	1 : 4.6	
9	"	"	"	"	5.00	1 : 5.0	
10	"	"	"	"	6.00	1 : 6.0	
11	"	"	"	"	7.00	1 : 7.0	
12	"	"	"	"	8.00	1 : 8.0	
13	"	"	"	"	9.00	1 : 8.0	
14	"	"	"	"	10.00	1 : 10.0	

* Chemistry of Pottery, p. 90.

For increasing the ratio of alumina to silica, the Logan clay was treated with increasing additions of Florida kaolin. A series was made starting with the Logan clay unadulterated, (in which the ratio of alumina to silica is 1 to 4.6,) and ending with trials made wholly of kaolin, with a ratio of alumina to silica of 1 to 2. This series was as follows:

SERIES II.

No.	KNaO	CaMgO	Al_2O_3	Fe_2O_3	SiO_2	Ratio Al_2O_3 to SiO_2	Remarks
8	.076	.161	1.00	0.06	4.60	1 : 4.60	
7	"	"	2.00	"	6 60	1 : 3.30	
6	"	"	3.00	"	8.60	1 : 2.86	
5	"	"	4.00	"	10.60	1 : 2.65	
4	"	"	6.00	"	14.60	1 : 2.45	
3	"	"	10.00	"	22.60	1 : 2.26	
2	"	"	20.00	"	42.60	1 : 2.13	
1	"	"	1.00	"	2.00	1 : 2.00	

The complete test covered by the two series is shown in the following table:

No.	KNaO	CaMgO	Al_2O_3	Fe_2O_3	SiO_2	Ratio Al_2O_3 to SiO_2	Remarks
1	....	....	1.00	....	2.00	1 : 2.00	Florida kaolin
2	.076	.161	20.00	0.06	42.60	1 : 2.13	Obtained by mixing Florida kaolin and Logan F. C.
3	"	"	10.00	"	22.60	1 : 2.26	
4	"	"	6.00	"	14.60	1 : 2.45	
5	"	"	4.00	"	10.60	1 : 2.65	
6	"	"	3.00	"	8.60	1 : 2.86	
7	"	"	2.00	"	6.60	1 : 3.30	
8	.076	.161	1.00	0.06	4.60	1 : 4.60	Logan F. C.
9	"	"	"	"	5.00	1 : 5.00	Obtained by additions of Flint to Logan F. C.
10	"	"	"	"	6.00	1 : 6.00	
11	"	"	"	"	7.00	1 : 7.00	
12	"	"	"	"	8.00	1 : 8.00	
13	"	"	"	"	9.00	1 : 9.00	
14	"	"	"	"	10.00	1 : 10.00	

INGREDIENTS FOR THE SYNTHETIC CLAYS.

The sand used in the above mixtures was glass sand, of 40 to 60 mesh fineness.

A set of brickettes was also made running from 9a to 14a inclusive, using 150 to 200 mesh sand, but retaining the same mixtures as in the 9 to 14. A third set was also made from 9b to 14b, using sand which would not settle in twenty minutes in a standard sedimentation can in the university laboratory.

The finest sand, that which would not settle in twenty minutes, was obtained by vigorously stirring up commercial potters' flint with water in the standard sendimentation can.

This can was about twenty-six inches high and twelve inches in diameter, with a spout or tubulature one and one-fourth inches in diameter, placed six inches from the bottom.* After allowing the flint and water to stand twenty minutes, the liquid was let off through the spout. This was repeated until practically all of the sand which would not settle in twenty minutes was drawn off. The milky liquid thus obtained was allowed to stand for several days, after which the clear water was drawn off and the fine flint was obtained and dried.

The 40 to 60 mesh was obtained by taking glass sand and throwing same on a 40 mesh screen; all that went through was subsequently thrown on a 60 mesh sieve, and all that remained on the 60 mesh was reserved for use. That which went through 60 or did not go through 40 was rejected.

The 150 to 200 mesh was obtained by taking the sand rejected in the preceding process and grinding in a ball mill for two hours, then putting through a 150 mesh and then a 200 mesh sieve. All that went through the 150 mesh and lodged on the 200 mesh was reserved for use; that which went through 200 was rejected, and that which did not pass through 150 was reground.

The kaolinite used was a Florida kaolin, 98 per cent. pure kaolinite. This was passed through a 20 mesh screen before use.

The following table gives the proportion of each ingredient used in making the brickettes:

* See Trans. Am. Cer. Soc., Vol. II, p. 104.

No.	Logan Clay Grams	Kaolin Grams	Sand		
			40 to 60 mesh	150 to 200 mesh	Floated
1	0.0	800.0	.....	.....	.....
2	60.5	739.5	.....	.....	.....
3	118.0	682.0	.....	.....	.....
4	190.0	610.0	.....	.....	.....
5	273.0	527.0	.....	.....	.....
6	350.0	450.0	.....	.....	.....
7	487.0	313.0	.....	.....	.....
8	800.0	0.0	.....	.....	.....
9	755.0	.....	55.0	.....	.....
10	744.0	.....	156.0	.....	.....
11	662.5	.....	237.5	.....	.....
12	596.5	.....	303.5	.....	.....
13	543.0	.....	357.0	.....	.....
14	498.0	.....	402.0	.....	.....
9A	755.0	.....	.....	55.0	.....
10A	744.0	.....	.....	156.0	.....
11A	662.5	.....	.....	237.5	.....
12A	596.5	.....	.....	303.5	.....
13A	453.0	.....	.....	357.0	.....
14A	498.0	.....	.....	402.0	.....
9B	755.0	.....	.....	.....	55.0
10B	744.0	.....	.....	.....	156.0
11B	662.5	.....	.....	.....	237.5
12B	596.5	.....	.....	.....	303.5
13B	543.0	.....	.....	.....	357.0
14B	498.0	.....	.....	.....	402.0

PREPARING THE SAMPLES.

Two test brickettes were made from each mixture. These brickettes were made in a wooden mould 2½ x 4½ x 1 inches. The ingredients in each case were mixed thoroughly in a granite-ware pan, then made into a stiff mud with distilled water. The brickettes were dried in the laboratory by simply putting on pallets on shelves.

After drying the brickettes they were placed in No. 12 kiln of the Hocking Fire Clay Company, at Logan, Ohio. This is a round down draft kiln in which they regularly do salt-glazing. One brickette of each number was placed well to the top of the kiln, and one of each number put four inches above the floor. The kiln was burned and salted in the usual manner, and with results of ordinary attainment.

RESULTS OF BURN.

No. 8 brickettes, made from the Logan clay, took a nice smooth salt-glaze, deep brown in color, similar to the usual run of glazes obtained at this plant. This will be used as a basis of comparison.

Very little difference was noticed between the samples burned at the top and those burned at the bottom of the kiln. The results of the test were practically the same for both sets of brickettes, and were as follows:

The brickettes below No. 8 (those in which the ratio of alumina to silica was increased) deteriorated rapidly in the quality of the glaze. No. 7 was almost as good as No. 8, but the glaze was a little inferior in finish. Nos. 5 and 6 had a very poor dull glaze, greatly inferior to that of No. 8, and not at all entitled to be called a successful salt-glaze. No. 6 was a little better than No. 5. No. 4 had taken a dull reddish-brown gloss with insufficient thickness, glossiness or finish to be called a glaze. No. 3 had lost still further the resemblance to a glaze, and a dull red surface, slightly glossy in spots was all that the salt had accomplished. Nos. 1 and 2 had merely been colored a brick red, and showed no glaze whatever. No. 1 had the appearance of a red face brick, having assumed a fine red color very uniformly.

This red color shown by the unglazed brickettes is probably due to the volatilization of Fe_2Cl_6 and the subsequent deposition of Fe_2O_3, which occurs in salt-glazing, as shown by Knett,* and its entering afterwards into silicate formation with the clay. Occasionally red clouds of Fe_2Cl_6 appear at the beginning of the operation of salt-glazing a kiln, but soon cease. It is this Fe_2Cl_6 which is deposited on the ware as Fe_2O_3. The surface of the brickettes showing this color presented a beautiful mass of crystals under the glass.

Of the brickettes above No. 8 (those in which the ratio of alumina to silica was gradually diminished) all had taken a good salt-glaze. The following facts were noted:

* Thonindustrie-Zeitung (1896).

(*a*) As to brightness and smoothness, the brickettes from No. 8 up to No. 14 showed no deterioration, but on the other hand an improvement. No. 14 was a much brighter glaze than No. 8. In this respect the different finenesses of sand made no apparent difference, all finenesses for each number being equally bright and smooth.

(*b*) In color, the glazes grew much lighter as the sand content increased. Each brickette, from No. 8 up, was a little lighter than its predecessor. From No. 8, which was a deep brown, the color lightened to a stone gray in No. 14. A noticeable fact was that in the brickettes containing the finer sands, the lightening effect was increased. For instance, No. 11-A was as light as No. 14, and in the A series the color reached its lightest, being a fine clear gray in No. 14-A. In the B series No. 10-B was about the same shade as No. 11-A, and from No. 10-B the color grew lighter, but with a greenish tinge so that, while No. 14-B was very light it was a greenish gray and not a clear gray, as in No. 14-A.

(*c*) As to the crazing of the glazes, the following list gives the crazed and uncrazed glazes:

No.	Uncrazed	Crazed
8	8A 8B	....
9	9A 9B	.....
10	.....	10A 10B (slight)
11	.....	11A 11B
12	....	12A 12B
13	.....	13A 13B } slight
14	.....	14A 14B }

As the foregoing series showed no deterioration of the salt-glaze with the increase of silica, contrary to what was expected, it was decided to still further increase the ratio of silica to alumina. For this purpose the following series was made:

SERIES III.

No.	KNaO	CaMgO	Al_2O_3	SiO_2	Ratio Al_2O_3 to SiO_2	Remarks
15	.076	.161	1.00	12.50	1 : 12.5	
16	"	"	"	15.00	1 : 15.0	
17	"	"	"	20.00	1 : 20.0	
18	"	"	"	40.00	1 : 40.0	
19	"	"	"	60.00	1 : 60.0	
20	0.0	0.0	0.0	100.00	all sand	

In view of the fact that in the previous test the fineness of sand made no apparent difference in the quality of the glaze the piece took; and also that samples in the bottom of the kiln showed no wide difference from those in the top, it was deemed sufficient to use merely one grade of sand in Series III, and to make only one set of brickettes. Accordingly, the fine floated silica was used, and one set of brickettes made as follows:

No.	Parts Logan Clay	Parts Sand
15	367	433
16	313	487
17	242	558
18	127	673
19	86	714
20	0	800

The brickettes were molded as those in Series I, except that owing to the extreme shortness of the mixtures, due to so much sand, it was necessary to use dextrine as a bonding agent to stick the material together and make brickettes which could be handled.

The brickettes were placed well to the top in one of the salt glazing kilns of the Hocking Clay Manufacturing Company, and burnt with a kiln of their ware. On drawing, the following results of the test were noted:

No. 15 was glazed but the glaze was thin, and in some places grew so thin as to be dull and rough. The glaze could not be called a good salt-glaze.

No. 16 showed glassy and smooth in places, but was mostly dull and rough, with insufficient glassiness to be called glazed.

No. 17 showed faint traces of glaze, but the greater part of the surface was dull, and while the salt had affected the entire surface, it had merely made a coating not unlike melted sugar in appearance. No. 17 presented a very interesting point, in that the brickette was well glazed on top but on none of the sides. No doubt, as the piece was exposed to flying ash and dust in the kiln, a certain amount of coal ash lodged on top of the brickette. This coal ash supplied the necessary alumina and the brickette the necessary silica to make a salt-glaze. The ash being unable to cling to the vertical sides of the brickette left these portions too silicious to make a glaze.

Nos. 18, 19 and 20 showed no traces of glaze whatever, and the salt had had no action on the pieces whatever. These brickettes had the same finish to their surface as when they entered the kiln, and very much resembled sand or bath bricks.

From Series I it was seen that a clay with an alumina-silica ratio of 1 to 10 gave a good salt-glaze. From Series III it was found that a clay with an alumina-silica ratio of 1 to 12.5 did not give a good salt-glaze. The limit lies between these two ratios, but lack of time prevented its exact determination. In view of this we shall have to assume temporarily that the limit was 1 to 12.5. This is more than likely, as the ratio of 1 to 10 gave a very good glaze, and the ratio of 1 to 12.5, while not a good glaze exactly, still it was not far over the line.

The following conclusions may now be safely drawn from the foregoing experiments in regard to the alumina-silica ratio.

1. A clay may be either too aluminous or too silicious to be successfully salt-glazed.

2. Clays containing alumina and silica between the molecular ratio of

1.00 alumina to 4.6 of silica
1.00 " " 12.5 "

are capable of receiving a salt-glaze if the process is properly carried out. If these limits are exceeded the material is not suited for salt-glazed ware.

3. While it is possible to salt-glaze clays containing alumina and silica in the above ratios, the requirements of the process of making stiff-mud goods would not permit the use of such silicious clays as those with an alumina-silica ratio of 1 to 9 or 1 to 10. That is, the practical value of a clay, as to plasticity, strength, etc., will draw narrower limits than those found in the above tests.

4. As regards brightness, smoothness and finish of the salt-glaze, it makes but little difference whether the free silica in the clay is fine or coarse.

5. As regards color, the finer the sand the lighter the color of the glaze. This has been substantiated in a practical way by Mr. A. Magoon, Superintendent of the Hocking Clay Manufacturing Co.

INFLUENCE OF EFFLORESCENCE.

Having come to some conclusion as to the limits of alumina and silica in a clay with reference to its receiving a good salt-glaze, it was decided to next look a little into the matter of efflorescence, or the presence of soluble salts in a clay and their bearing on the salt-glazing question.

Referring again to the difference in the lime contents of the Logan clay and the salt-glaze, as shown by the analyses, it is to be noted in this case that whatever efflorescence comes to the surface of the clay in burning did not interfere seriously with the clay taking a good glaze. The analysis of the glaze indicates that soluble salts were absorbed and became a part of the glaze.

Professor Orton * speaks of lime salts in paving brick interfering with their being salt-glazed, and many practical sewer-pipe burners will tell you of the troublesome "scum" which makes salt-glazing harder to accomplish.

The question now arises as to what extent the salt-vapors are able to attack efflorescence and secure the needed

* Ohio Geol. Survey, Vol. VII, Pt. I, p. 176.

alumina and silica from the clay beneath, and what amount of soluble salts in a clay will interfere seriously with salt glazing it.

In order to make an experiment in this direction some of the Logan fire clay was treated repeatedly with distilled water and finally with hot distilled water, the water being siphoned off after each treatment until it was judged that whatever soluble salts the clay contained had been dissolved out and drawn off in the water. The clay was then dried and ground, and put through a 20 mesh sieve. Starting with this clay soluble salts were added systematically up to three percent. The soluble salts added were calcium sulphate and magnesium sulphate in the proportion of three parts of the former to one of the latter.

The series was as follows:

SERIES IV.

No.	Logan Clay free from Soluble Salts	$CaSO_4$	$MgSO_4$	Per Cent. Soluble Salts
1-E	796	3.00	1.00	0.5
2-E	792	6.00	2.00	1.0
3-E	788	9.00	3.00	1.5
4-E	784	12.00	4.00	2.0
5-E	780	15.00	5.00	2.5
6-E	776	18.00	6.00	3.0

Two sets of these brickettes were made, one set was kept in the laboratory to dry, in order to note the appearance of efflorescence, if any, in drying. The other set was placed while damp in one of the Logan kilns, and burned.

The set of brickettes dried in the laboratory showed merely traces of efflorescence which did not increase with the amount of soluble salts contained in the clay.

The brickettes burned and salt-glazed all showed a good, smooth, deep brown glaze. The brightness of the glaze increased with the amount of soluble salts. The color of the glaze in each case was remarkable for its rich brown tone, being a deeper color than is usually obtained in the regular run of goods from these kilns.

Whether the soluble salts which had been added to the clay were brought to the surface in burning, is a question which cannot be decided with the data at hand. The set put in the laboratory to dry, however, showed that the efflorescence did not come out to any extent in drying.

The effect of efflorescence therefore is still a matter to be decided. That enough calcium sulphate can exist on the surface of a brick to prevent its taking a good salt glaze under standard conditions, the writer has demonstrated by heavily coating a number of green bricks at the Logan factory with plaster of paris mixed with water, and placing same in different parts of the kiln. The surfaces which were treated in this manner came out slightly glazed, but with a dull yellowish, unsightly color.

SALT GLAZE CONSIDERED AS A GLASS.

Knett gives the following as the reactions which occur in salt-glazing:

$$(Al\ Fe)_2O_3 \times SiO_2 + 6\ Na\ Cl = (Al\ Na_3)_2O_3 \times SiO_2 + Fe_2Cl_6$$
$$Fe_2Cl_6 + 3\ H_2O = Fe_2O_3 + 6\ HCl$$

According to this, the ratio of soda to alumina in the glaze would be

$$3\ Na_2O : Al_2O_3$$

Langenbeck * says, "Such glazes (salt-glazes) approximate a formula:

$$1\ RO,\ .5\ R_2O_3,\ 4.\ SiO_2$$

By referring to formula III, deduced from the actual analysis of the Logan salt-glaze, namely:

$$\left.\begin{matrix}1.328\ Na_2O\\ .008\ K_2O\\ .297\ CaO\\ .008\ MgO\end{matrix}\right\} 1.000\ Al_2O_3 \left\{ 4.418\ SiO_2 \right.$$
$$.078\ Fe_2O_3$$

It is readily seen that both Knett and Langenbeck place the ratio of alumina to soda much lower than that found by the writer to actually exist in the glaze.

In order to prove the feasibility of glasses high in alumina, it was decided to make up a series of glasses to substantiate the glaze formula found by analysis.

*Chemistry of Pottery.

In a series of glass experiments Professor Orton * demonstrates that a glass of the following composition:

$$\left.\begin{matrix}.5\ Na_2O \\ .5\ CaO\end{matrix}\right\} 0.20\ Al_2O_3,\ 0.80\ SiO_2 \qquad \text{Oxygen Ratio } 1:1$$

was much more fusible at cone 8 than a glass of the composition:

$$\left.\begin{matrix}.5\ Na_2O \\ .5\ CaO\end{matrix}\right\} 0.0\ Al_2O_3,\ 0.50\ SiO_2 \qquad \text{Oxygen Ratio } 1:1$$

Starting with the glass given in the above formula, it was decided to not merely increase the alumina as Professor Orton had done, but to replace the lime with alumina in order to approach the salt-glaze formula, which is practically:

$$\left.\begin{matrix}.8\ Na_2O \\ .2\ CaO\end{matrix}\right\} 0.6\ Al_2O_3,\ 2.70\ SiO_2 \qquad \text{Oxygen Ratio } 1:1.93$$

To this end the following series of glasses was made up:

SERIES V.

No.	Na_2O	CaO	Al_2O_3	SiO_2
1	0.5	0.5	0.0	0.5
2	0.5	0.4	0.033	0.5
3	0.5	0.3	0.066	0.5
4	0.5	0 2	0.100	0.5
5	0.5	0.1	0.133	0.5
6	0.5	0.0	0.166	0.5

In this series the oxygen ratio was kept constant, namely, 1 : 1.

These glass mixtures were placed in porcelain cups and fired in a test kiln to cone No. 8. On cooling and drawing the kiln the following results were noted:

(*a*) No. 1 was a stony mass, resembling blast furnace slag.

(*b*) Nos. 2 to 6, inclusive, were all good glasses, well melted.

(*c*) No. 5 was a little better glass than No. 6, showing lime had helped the mixture to some extent.

(*d*) No. 5 was as good, if not a little better than any of the other glasses.

* Trans. Am. Cer. Soc., Vol. IV.

Taking the formula for No. 5 glass, namely:

$$\left.\begin{matrix}.5\ Na_2O \\ .1\ CaO\end{matrix}\right\} .133\ Al_2O_3,\ 0.50\ SiO_2$$

and reducing this to a basis of RO = 1, we obtain

$$\left.\begin{matrix}.83\ Na_2O \\ .17\ CaO\end{matrix}\right\} .221\ Al_2O_3,\ .83\ SiO_2$$

It will be seen that the RO contents here are very close indeed to the RO contents of the Logan salt-glaze.

Having proved that a soda-alumina glass is fully as feasible as a soda-lime glass, and having obtained in the No. 5 glass an RO content practically the same as in the salt-glaze, a series was next prepared, increasing the alumina and silica contents of No. 5 glass and also the oxygen ratio, until the formula for the salt-glaze was obtained.

The series for this purpose was as follows:

SERIES VI.

Number	Na_2O	CaO	Al_2O_3	SiO_2	RO	Remarks
7	0.80	0.20	0.221	.83	1 : 1.00	
8	"	"	0.360	1.30	1 : 1.25	
9	"	"	0.440	1.76	1 : 1.50	
10	"	"	0.510	2.23	1 : 1.75	
11	"	"	0.600	2.70	1 : 1.93	Logan Salt-Glaze

This series was treated as the preceding, being melted in porcelain cups at cone No. 8.

On cooling and drawing the following results were noted:

(*a*) All had melted to glasses.

(*b*) Nos. 7, 8, 9 and 10 had melted into solid cakes of glass, showing very little difference.

(*c*) Nos. 7 and 8 showed no bubbles in the glass. No. 9 showed a number of small bubbles through the glass mass, and No. 10 showed larger bubbles. But these were still very good glasses.

(*d*) No. 10 while a glassy mass, very much the same in color as the Logan salt-glaze, was very spongy throughout. The mass, however, was a good, sharp, well melted glass, which, if ground up and applied to a brick surface, would

undoubtedly flow to a glaze very similar to the salt-glaze. The bubbling was merely due to insufficient time in melting to allow the carbon gases to completely escape.

By the foregoing glass experiments it has been clearly demonstrated.

First. That in a soda-lime glass similar to that shown by No. I, Series V, alumina may replace the lime in part or even completely, and a good glass will still be obtained. In other words, soda-alumina glass is as feasible as a soda-lime glass at the same heat.

Second. That a little lime in a glass mixture of this type makes a little better glass than with no lime.

Third. That a soda-lime alumina glass can be made at cone No. 8 with a wide range of alumina and silica. Glassy masses are obtained with mixtures in which the alumina and silica runs from 0.221 Al_2O_3, 0.83 SiO_2 to 0.600 Al_2O_3, 2.70 SiO_2.

Fourth. That by compounding a mixture corresponding to the formula deduced from the analysis of the salt-glaze, a a glass is obtained at salt-glazing temperature (cone No. 8), very similar in all respects to the salt-glaze actually obtained in commercial work. The analysis of the salt-glaze and the subsequent compounding of a glass from this analysis therefore harmonize perfectly.

SUMMARY.

In the above paper, we have obtained the analysis and formulæ for both the clay and glaze actually used in successful salt-glazing work. From the experiments it was found that clays with a wide molecular ratio of alumina to silica could be salt-glazed, viz.: from 1 : 4.6 to 1 : 12.5. Also, from the glass tests, we found that glasses could be made with a generous variation of alumina and silica. Connecting these observations, it is reasonable to suppose that the salt-glaze on the different clays varied in composition according to the clay composition. For instance, in the Logan clay and glaze, the ratio of alumina to silica is very close. Now when the ratio in the clay had been changed, is it not probable that the composition of the glaze formed changed, also ?

This can only be determined by actually analysing the salt-glaze formed on different bodies of known composition.

By the efflorescence test it was demonstrated that up to three per cent. of soluble salts may be present in clay without seriously interfering with salt-glazing when conducted at cone No. 8. Whether or not, however, all the added salts came to the surface was not determined.

It would be an interesting point for other investigators in this line, to determine the effect of sulphurous gases on salt-glazes. This could be done by treating a piece of salt-glazed ware with sulphur gases in a closed tube. That the action of sulphur has an important bearing on the quality of the salt-glaze seems highly probable. It would be of interest and value to also determine the temperature at which glazing begins, and whether it can be overdone by high heat. Also to ascertain the relation of the temperatures of salt-glazing red-burning clays and buff-burning clays, and what effect $CaSO_4$ or other soluble salts has on the glazing of red-burning clays. Furthermore, whether steam aids the process or not is still a questionable point.

The writer regrets that the time set apart for this work on salt-glazing did not permit of his looking into the last mentioned points, but hopes that they will be investigated before long.

In conclusion, the writer desires to thank especially Mr. A. Magoon, of the Hocking Clay Manufacturing Company, who generously took the time and trouble to furnish samples of his clay and glaze, and to take care of the burning of the experimental trials.

THE CONSTITUTION OF CHROMIUM-TIN PINK.

BY
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The subject of the chromium-tin colors offers a field for investigation to which, considering the extent to which the colors are used in ceramic work, but little attention has been paid. Most of the published information on the subject we owe to Leykauf and Seger. The former studied the compounds of chromium and tin from a purely chemical standpoint, while Dr. Seger has given receipts and directions for the production of ceramic colors by the use of these materials.

REVIEW OF PREVIOUS INVESTIGATIONS.

Leykauf concluded from his investigations, (Jour. Soc. Chem. Ind., 1892) that chromium stannate can be obtained by igniting stannic chromate at a high temperature, and forms a dark violet mass, from which glazes varying in color from rose-red to dark violet can be prepared. He found that when chromic oxide and stannic oxide were ignited together in the proportion of one of the former to fifty of the latter, that a mass composed of a mixture of fine crystals and fused, glassy particles of a beautiful permanent lilac color were obtained. According to Leykauf, "pink color" consists of this substance, mixed with calcium stannate.

Leykauf's statement that "pink color" contains chromic stannate was contradicted by Petrik, who found that a pink coloring body could be produced as well by the aid of alumina or a mixture of alumina and magnesia, as by means of tin oxide. He used for the purpose the precipitated hydroxides obtained from 90.6 g. ammonia alum, and 24.6 g. magnesium sulphate, which were ground together with .5 g. potassium chromate. The mass was burned twice, being well washed after the first burning. During the second burning the red color appeared. In the presence of a larger amount of chromic oxide, only a green color resulted. An increase in the amount of magnesia favored the formation of the red color, while the substitution of lime, baryta or strontia for magnesia

seemed to have a contrary effect. Petrik thought that this was the case because the precipitated alumina was coarser, and that, in all probability, the chromium color compound only appears pink when very finely divided. From this investigation, Petrik concluded that the "pink color" does not contain chromic stannate, but that a red chromium compound, apparently chromic oxide in a finely divided state, is precipitated upon the stannic acid serving as a "base."

The investigations of Leykauf and Petrik are of considerable interest, but the hypotheses which they offered in explanation of the formation of the color are evidently contradictory, and the facts produced in each case were insufficient to establish any theory.

If we lay aside the theoretical considerations and look for the useful facts which have been published on the subject, we find them stated briefly in Seger's discussion of underglaze colors, as follows:

"Chromium oxide with tin oxide and lime produces red tints. The red colors show many peculiarities which must be considered. They are obtained, when to a mixture of marble and tin oxide a small addition of potassium chromate is made, and the mixture strongly ignited. Outside of the potassium chromate, quite a number of substances produce the same effect, but it is safest to work with potassium chromate. However, if too much of the latter is used, (over 3 to 4 per cent,) the resisting power of the underglaze color is reduced considerably. If silica is added to the mixture the color turns toward red, if the lime is decreased, toward lilac, and excluding the latter from the composition entirely, it becomes a pure lilac color. A very strong fire is necessary. The red colors require oxidizing conditions, and are destroyed by reduction but renewed on continued oxidation."

In addition to this general discussion, Seger gives, with directions for their preparation, the following receipts:

Pink-Red.

	Parts
Pure ignited tin oxide	50
Marble	25
Quartz	18
Acid potassium chromate	3
Borax	4
	100

This fritt, after ignition, would have the following formula:

$$\left.\begin{matrix}.924\ CaO\\ .038\ K_2O\\ .038\ Na_2O\end{matrix}\right\} .038\ Cr_2O_3 \left\{\begin{matrix}1.109\ SiO_2\\ 0.076\ B_2O_3\\ 1.238\ SnO_2\end{matrix}\right.$$

Reddish-Blue.

	Parts
Ignited tin oxide	70
Marble	23
Acid potassium chromate	3
Borax	4

This fritt, after ignition, would have the following formula:

$$\left.\begin{matrix}.918\ CaO\\ .0406\ K_2O\\ .0414\ Na_2O\end{matrix}\right\} .0406\ Cr_2O_3 \left\{\begin{matrix}.0828\ B_2O_3\\ 1.875\ SnO_2\end{matrix}\right.$$

By leaving out the marble, a lilac is obtained which is suitable for white ware, but cannot resist a high heat like that of the porcelain kiln.

Lilac.

	Parts
Ignited tin oxide	50
Potassium bichromate	3
Borax	20

of which the formula would be:

$$\left.\begin{matrix}.163\ K_2O\\ .837\ Na_2O\end{matrix}\right\} .163\ Cr_2O_3 \left\{\begin{matrix}1.674\ B_2O_3\\ 5.36\ SnO_2\end{matrix}\right.$$

The first of these receipts is used by many potters, not only as an underglaze color, but also in majolica glazes. However, in some cases considerable difficulty is experienced in using it, owing to the fact that the glazes destroy the color. An example of this sort of difficulty, and the manner in which it was overcome, is found in the paper by Mayer, in the Transactions of the American Ceramic Society, Vol. I. Mr. Mayer's experiments proved conclusively that in his case the trouble was caused by the glaze, and that another glaze could be used, which, under the same conditions, did not destroy the underglaze color. Why one glaze destroyed the color and another did not, has not been determined.

SCOPE OF THIS INVESTIGATION.

The experiments described below were made for the purpose of adding something to the available information upon the following phases of this subject: *First.* The most

favorable composition for the pink fritt. *Second*, The type of low fire glaze which develops the color to the best advantage.

THE MOST FAVORABLE COMPOSITION OF THE PINK FRITT.

The work done on the first phase of the subject, consisted of making three series of fritts, beginning with a series of combinations of chromic oxide and stannic oxide alone. Taking the members of this series which had the highest allowable ratio of chromic oxide to stannic oxide, a second series was made, keeping this chrome-tin ratio constant, and adding an increased proportion of lime. Finally, the best member of the chrome-tin-lime series was taken as a basis for a third series, in which silica was added in regularly increasing proportions.

In these series, the tin was supplied by SnO_2, the chromium by CrO_3 and the lime by $CaCO_3$. These materials were all chemically pure, but for the silica, Golding flint was used. The SnO_2, $CaCO_3$ and SiO_2 were all fine enough to make further grinding unnecessary, the coarsest leaving about 3 per cent. on an 80 mesh screen. In making the mixtures, the insoluble ingredients for each fritt were weighed dry, and placed in a caserole. The CrO_3 was added from a burette, a standard solution being used. Sufficient water was then added to make the mass into a thin paste, which was thoroughly stirred and then dried on a water bath, the stirring being repeated at frequent intervals to insure an intimate mixture of the ingredients. Each mixture was then placed in an air bath to complete the drying, and finally each was thoroughly ground in an agate mortar. Owing to the fineness of the materials used, it was possible to secure a good fine mixture with a reasonable amount of grinding, and it is thought that by adding CrO_3 in solution, a better contact of the particles was secured than could have been done by a dry process of mixing.

The fritts were calcined in a small gas furnace, fired by means of a blast lamp. The furnace consists of a vertical hollow cylinder of fire clay, the inside diameter being about five inches and the height six inches. The blast lamp flame

enters horizontally at the bottom of the furnace. The cylinder is covered with a fire clay disc, in the center of which is an inch hole which serves the double purpose of a peep hole and gas outlet. With this furnace we were able to reach cone 18 with oxidizing conditions. The fritts, about 30 g. each, were calcined in small fire-clay crucibles without lids. Each member of the first two series was fired for forty-five minutes, at about cone 16. The third series was kept at cone 8 to 10 for an hour.

The following table shows the composition of the members of the first series:

SERIES A-I.

No. of Fritt	Composition by Weight		Equivalents.	
	Parts of SnO_2	Parts of CrO_3	SnO_2	Cr_2O_3
1	50	1	1	.015
2	40	1	1	.0185
3	30	1	1	.025
4	20	1	1	.0375
5	15	1	1	.050
6	10	1	1	.075
7	5	1	1	.150
8	2.5	1	1	.300

The appearance of the fritts of the first series, after calcination, were as follows:

Each fritt was in a soft friable mass, none of them having formed a hard cake. The color of the first four was a rich purple, nearly as strong in one as in the other. Beginning with No. 4, there came a regular gradation of color, ranging from a rich purple to the green of Cr_2O_3. This showed that with these conditions the maximum allowable molecular ratio of Cr_2O_3 to SnO_2 was .0375 : 1. Above this ratio the excess of SnO_2 was not great enough to prevent the existence of free Cr_2O_3.

No. 4 of this series was allowed to cool very slowly in order to favor the formation of crystals, and was then examined microscopically. The microscope revealed isolated white particles and colored masses, which showed no distinct granular or crystalline particles. When viewed through crossed Nicol's prisms, no evidence of crystallization could

be detected. This does not correspond to the description given by Leykauf. It is true that the white particles looked like crystals, but they failed to show any evidence of polarization.

For the second series, the ratio of SnO_2 to CrO_3 was maintained the same as that in No. 4 of the first series, and the lime was increased as shown in the following table:

SERIES A-II.

No. of Fritt	Percents by Weight		Molecular Proportions		
	Fritt No. 4	$CaCO_3$	SnO_2	CaO	Cr_2O_3
9	100	0	1	.0	.0375
10	80	20	1	.391	.0375
11	60	40	1	1.04	.0375
12	40	60	1	2.84	.0375
18	20	80	1	6.25	.0375

The effect of the lime upon the chrome-tin fritt was very marked. No. 10 was a reddish-brown, while No. 11 was a stronger, deeper brown. The higher numbers became glassy, and the color took on a greenish cast, which predominated in the highest numbers. No. 13 melted to a green glass, which attacked the crucible strongly.

Taking the chrome-tin-lime ratio of 11, the effect of SiO_2 was then tried by the following series:

SERIES A-III.

No. of Fritt	Molecular Proportions			
	SnO_2	$CaCO_3$	SiO_2	Cr_2O_3
14	1	1	.0	.0375
15	1	1	.5	.0375
16	1	1	1.0	.0375
17	1	1	1.5	.0375
18	1	1	2.0	.0375

In this series the colors ranged from a strong brown in No. 14 to a strong pink in No. 18. The difference in shade between Nos. 16, 17 and 18 was, however, very slight, and any of these would pass for a good pink fritt.

It would require a much more extended investigation than this to determine to what extent the proportions of these fritt ingredients could be varied, and a good fritt be obtained. The following consideration made it seem desirable to use, in the glazes to be tried, a formula not differing widely from that of the upper numbers of Series A-III:

1. The tin might be raised without detriment to the fritt itself, but it is not considered desirable to introduce more tin than is necessary into soft fire glazes. It may be that the tin could be decreased, but the results of these experiments do not indicate that this could be done to any considerable extent.

2. Series A-I indicates that the quantity of chromic oxide is as high as is allowable.

3. The lime was purposely made as high as Series A-II would warrant.

4. This fritt corresponded closely, both in appearance and in composition to the pink fritt recommended by Seger, except that the tin is lower in the fritts of Series A-III. Seger's fritt, however, was intended for an underglaze color for porcelain, and in that case a high excess of tin in the fritt would not be detrimental to the glaze.

THE EFFECT OF LEAD AND BORACIC ACID UPON THE CHROMIUM-TIN PINK IN LOW FIRE GLAZES.

As has been stated, it was desired to determine what type of low fire glaze is most favorable for the development of the pink color. In planning this investigation, the following facts were to be taken into consideration:

1. Too high a lead content in the glaze is detrimental to the color. Professor Orton, in one of his lectures on the subject of colored glazes, states that in a chromium-tin pink glaze not more than .45 equivalents of lead is permissible. Moreover, preliminary tests of my own with a glaze containing .6 equivalents of lead failed to produce a pink glaze at all.

2. Seger states that boracic acid in the fritt changes the color toward purple, and it was expected that boracic acid in the glaze would produce the same effect to some extent.

In view of these facts the problem seemed to resolve itself into the following phases: *First*, To determine the permissible content of lead. *Second*, To find how much B_2O_3 is allowable. *Third*, To design a glaze maturing at cone 02 or 01, in which these limits of composition are not exceeded.

The first thing which was attempted in this connection was to determine the effect of increasing the content of boracic acid in a glaze containing lead. The glaze selected for this purpose had the following formula:

$$\left.\begin{matrix}0.25\ PbO\\0.25\ KNaO\\0.50\ CaO\end{matrix}\right\}\ 0.2\ Al_2O_3,\ 2.00\ SiO_2\ \left\{\begin{matrix}0.005\ Cr_2O_3\\0.185\ SnO_2\end{matrix}\right.$$

It was thought that .25 equivalents of lead was not sufficient to be detrimental to the color, and that quantity was accordingly used to help in softening the glaze.

The boracic acid was introduced into the glaze by the use of a fritt of the following composition:

$$\left.\begin{matrix}0.5\ Na_2O\\0.5\ CaO\end{matrix}\right\}\ 0.054\ Al_2O_3\ \left\{\begin{matrix}2.29\ SiO_2\\1.19\ B_2O_3\end{matrix}\right.$$

The pink fritt used in this series was the underglaze pink recommend by Seger:

$$\left.\begin{matrix}0.924\ CaO\\0.038\ K_2O\\0.038\ Na_2O\end{matrix}\right\}1.109\ SiO_2\ \left\{\begin{matrix}1.288\ SnO_2\\0.038\ Cr_2O_3\end{matrix}\right.\ 0.076\ B_2O_3$$

The composition of the glazes of this series is shown by the following table:

SERIES B.

No.	PbO	KNaO	CaO	Al_2O_3	SiO_2	B_2O_3	Cr_2O_3	SnO_2
1	.25	.25	.50	.2	2.00	.35	.005	.185
2	.25	.25	.50	.2	2.00	.40	.005	.185
3	.25	.25	.50	.2	2.00	.45	.005	.185
4	.25	.25	.50	.2	2.00	.50	.005	.185

It will be seen that the boracic acid varies between somewhat narrow limits in this case, but the range proved to be wide enough for the purpose.

The method of preparation of these glazes and of all those which will be discussed, was as follows:

The batches for the first and last glaze of each series were weighed out and ground wet in porcelain ball mills. After grinding, the glaze batches were screened through a 150 mesh screen. As most of the materials were finely ground to start with, two or three hours of grinding were sufficient in all cases, to make the glaze slip go through the 150 mesh sieve like so much water. After screening, the slip was allowed to settle and the supernatant water syphoned off as closely as possible. A sample of each slip was then taken and the quantity of dry material per unit of slip determined. This completed the data necessary to calculate the proportions in which the two slips would have to be blended to produce the intermediate numbers of the series.

The manner of the preparation of the fritts, except in the case of the pink fritts, for which a description has already been given, was to grind the fritt batch dry to secure a good mixture, and melt it in a large Hessian crucible in the gas furnace already described. The fritt when melted, was poured from the crucible into cold water, replacing and refilling the crucible and repeating the operation as many times as necessary. Fritts could be poured in this way which would not readily run through an opening in the bottom of the crucible. After being crackled in this way, a fritt would be ground wet in a glaze mill, screened through 150 mesh sieve, allowed to settle and the water syphoned off. The fritt was then dried ready for weighing.

The burning, unless otherwise stated, was done in a down-draft test kiln. With this kiln it was possible to get a fairly uniform distribution of the heat and to make as good a burn as can be expected with a test kiln having but one furnace. Of course, there were short periods of reduction, when fresh fuel was added, but this by careful firing could be kept from going too far. Coke was used for fuel in each burn until a red heat was reached. At that stage the use of coal was commenced. However, no fresh coal was thrown upon the grate bars, but was placed upon the coking plate just inside the furnace door and kept there till most of the volatile part of the fuel was roasted out before spreading

over the bars. In this way the condition of the kiln gases was kept fairly uniform.

The glazes were all applied to white tiles, 4½ inches long by 1 inch in width and set in saggers, which were then covered and luted tight.

The trials of series B were set on end in a large sagger and placed near the top of the kiln. Cones were placed in front of the sagger in a smaller sagger placed on its side. In burning, the heat was raised as rapidly as it could be and still keep the predominating conditions of the kiln strongly oxidizing. Cone 04 was reached in eleven hours. The heat was brought up more slowly during the next hour and the firing discontinued at the end of the twelfth hour, with cone 02 down in the front sagger, 01 having been reached in the sagger with the trials.

In the resulting glazes, the effect of the boracic acid was apparent. As was expected, the boracic acid softened the glaze, but also seemed detrimental to the color.

The appearance of the individual pieces was as follows:

Glaze B-1.

B_2O_3=.35—This glaze was somewhat immature and showed a tendency to bead up, leaving patches of bare surface on the tile. Slight tendency to flow. The color was a deep, strong maroon.

Glaze B-2.

B_2O_3=.40—Glaze fairly well matured, with some tendency to flow. Glaze would pass aside from color. Color, slightly inferior to B–1, there being some white flocky segregrations which are detrimental. Shade nearly the same as that of B–1.

Glaze B–3.

B_2O_3=.45—This is a good nature glaze but its appearance is bad. The flocky segregations, apparently due to tin, predominate.

Glaze B–4.

B_2O_3=.50—In this glaze the pink is almost entirely destroyed and the tin-enamel effect is solid instead of flocky.

It is evident that in this glaze, the use of more than .4 equivalents of boracic acid is not permissible. It is also evident that, under the conditions, at least .4 B_2O_3 is required in order that the glaze will mature. Moreover, the color is not bright enough to be satisfactory in any of the pieces.

The only definite conclusion that can be drawn from this series is that this particular glaze, (B–1) does not develop a good pink color when softened with sufficient B_2O_3 to make it mature under the conditions described at cone 0I.

In order to determine more definitely the effect of different equivalents of PbO and B_2O_3 upon the color, three series of glazes were made, being so designed as to keep the acid ratio and the alumina content constant throughout. In one case, SiO_2 alone was used for the acid, and in the RO group the ratio of KNaO to CaO was kept approximately 1 : 2, while the content of PbO increased regularly by additions of .2 equivalents. In the other series the RO group was maintained constant, while the SiO_2 was replaced by successive additions of .2 equivalents of B_2O_3. Finally a series was made in which the acid ratio was kept constant, but the PbO was made to decrease, while the B_2O_3 increased. The extremes for these series were as follows:

1. A fritted soda-lime glaze free from both PbO and B_2O_3:

$$\left.\begin{matrix}0.33\ KNaO\\0.67\ CaO\end{matrix}\right\}\ 2.00\ Al_2O_3,\ .2\ SiO_2\ \left\{\begin{matrix}0.005\ Cr_2O_3\\0.144\ SiO_2\end{matrix}\right.$$

In making this glaze, a fritt of the following composition was used, obviating the use of much raw material, and thereby softening the glaze considerably:

$$\left.\begin{matrix}0.33\ KNaO\\0.67\ CaO\end{matrix}\right\}\ 0.18\ Al_2O_3,\ 1.00\ SiO_2$$

2. A raw lead glaze without B_2O_3:

$$\left.\begin{matrix}0.851\ PbO\\0.011\ KNaO\\0.138\ CaO\end{matrix}\right\}\ 0.2\ Al_2O_3,\ 2.00\ SiO_2\ \left\{\begin{matrix}0.005\ Cr_2O_3\\0.144\ SnO_2\end{matrix}\right.$$

In this glaze, the CaO and KNaO were only such as were necessarily brought in by the pink fritt.

3. A fritted boracic acid glaze without lead:

$$\left.\begin{matrix}0.33\ KNaO\\0.67\ CaO\end{matrix}\right\}0.2\ Al_2O_3\ \left\{\begin{matrix}1.00\ SiO_2\\1.00\ B_2O_3\end{matrix}\right\}\begin{matrix}0.005\ Cr_2O_3\\0.144\ SnO_2\end{matrix}$$

In the batch for the glaze the amount of raw material was reduced to .05 equivalents of clay, by using the following fritt:

$$\left.\begin{matrix}0.373\ KNaO\\0.622\ CaO\end{matrix}\right\}0.176\ Al_2O_3\ \left\{\begin{matrix}.858\ SiO_2\\1.170\ B_2O_3\end{matrix}\right.$$

In these three series a pink fritt of the following composition was used:

$$\left.\begin{matrix}0.924\ CaO\\1.109\ SiO_2\\0.038\ K_2O\\0.076\ B_2O_3\\0.038\ Na_2O\end{matrix}\right\}\begin{matrix}1.238\ SnO_2\\0.038\ Cr_2O_3\end{matrix}$$

The following tables contain the formulæ of the glazes obtained by blending the extremes:

SERIES C.

No.	PbO	KNaO	CaO	Al_2O_3	SiO_2	Cr_2O_3	SnO_2
1	.000	.330	.670	.2	2.0	.005	.144
2	.200	.254	.544	.2	2 0	.005	.144
3	.400	.180	.420	.2	2.0	.005	.144
4	.600	.105	.294	.2	2.0	.005	.144
5	.851	.011	.138	.2	2.0	.005	.144

SERIES D.

No.	KNaO	CaO	Al_2O_3	SiO_2	B_2O_3	Cr_2O_3	SnO_2
1	.33	.67	.2	2.0	0.0	.005	.144
2	.33	.67	.2	1.8	0.2	.005	.144
3	.33	.67	.2	1.6	0.4	.005	.144
4	.33	.67	.2	1.4	0.6	.005	.144
5	.33	.67	.2	1.2	0.8	.005	.144
6	.33	.67	.2	1.0	1.0	.005	.144

SERIES E.

No.	PbO	KNaO	CaO	Al_2O_3	SiO_2	B_2O_3	Cr_2O_3	SnO_2
1	.851	.011	.138	.2	2.0	0.0	.005	.144
2	.681	.075	.244	.2	1.8	0.2	.005	.144
3	.511	.139	.350	.2	1.6	0.4	.005	.144
4	.340	.203	.456	.2	1.4	0.6	.005	.144
5	.170	.266	.563	.2	1.2	0.8	.005	.144
6	.000	.330	.670	.2	1.0	1.0	.005	.144

The glazes of these series were prepared, dipped, set and burned as described for series B.

The results obtained, while they failed to give the color that was desired, demonstrated unmistakably the effects of lead and boracic acid in the glaze.

Series C showed a regular gradation from a fairly good pink color in C–1 to a glaze in C–5, in which there was no resemblance whatever to a pink color. Individually, the glazes were as follows:

Glaze C–1.

PbO=0—Glaze immature. Color pink, but dull, owing to immaturity of glaze. This same glaze when fired to cone 3, gave a good clear pink.

Glaze C–2.

PbO=.2—Very bright and pretty pink enamel over greater portion of surface, surrounded by a border of white enamel. The glaze is somewhat immature. The same glaze was burned at cone 3 and though over-burned, gave a strong pink color with a fringe of white.

Glaze C–3.

Pbo=.4—Strangely enough, this number shows more of a tendency to bead up and have bare spots on the surface of the tile. The appearance is that of an immature enamel, fringed with white, with a flush of pinkish brown over the remainder of the surface. The shade is much darker than in C–2.

Glaze C–4.

PbO=.6—A gray opaque enamel, beaded to some extent. It is found by breaking the piece so as to show it in cross-section, that this enamel is mature only at the surface, beneath which the color is not destroyed.

Glaze C–5.

PbO=.851—Very ugly enamel with a tinge of green. From this series it is evident in the first place that none of the glazes will mature at cone 01 with the sort of burn described; and in the next place that for this glaze the upper limit of PbO lies somewhere between .2 and .4 equivalents. In this series, as also in the one preceding it, the

quantity of pink fritt was too great. The colors, where not destroyed, were unnecessarily strong, the glazes too opaque, and naturally more flux was required to produce a mature glaze than would be necessary if less of the pink fritt were used. It is thought that C–2 and C–3 would be considerably improved by using only enough pink fritt to introduce .003 instead of .005 equivalents of Cr_2O_3.

In Series D we have the effects of B_2O_3 even more clearly demonstrated. The glazes change progressively from immature pinks in D–1 through a series of mature purplish pinks, to a dark purple glaze, which is very immature at the other extreme.

Glaze D–1.

Same as C–1.

Glaze D–2.

B_2O_3=.2—This glaze is somewhat immature. The color though not fully developed, is a deep strong maroon. The same glaze fired to cone 3 resulted in a good smooth glaze with a strong pink color, slightly tinted with purple.

Glaze D–3.

B_2O_3=.4—This glaze is badly scummed but is perfect in other respects. The color is a strong dark pink with a marked purplish tint.

Glaze D–4.

B_2O_3=.6—Glaze not quite mature. In appearance very bad. It has a mottled effect produced by flocky seggregations of a pale purple color, replacing the pink. In this glaze the B_2O_3 is evidently too high.

The next two numbers, D–5 and D–6, are both far beyond the limit. Both are immature, and a very dark purple in color.

D–3 is by far the best number in this series. On one side of D–3 in the series, the lower limit of B_2O_3 is marked by D–2 with .2 B_2O_3, while on the other side the upper limit is passed before D–4 with .6 B_2O_3 is reached.

This series leads us to the following conclusions:

1. That B_2O_3 gives a purple tint to the pink color.
2. That in this glaze the quantity of B_2O_3 that can be

used is below .6 equivalents. The results of this series are consistent with those of series B, in which the best glaze contained .4 B_2O_3. The softening effect of the fritting in series D is obtained in series B by the use of .25 PbO. The color is better in the glaze without lead, and yet the comparison indicates that as much B_2O_3 is permissible in one case as in the other.

Series E. In view of the results obtained in Series C and D it is apparent, that no member of Series E can be satisfactory; for as we go down the list in the table, we see that before we get below the upper limit of PbO, we pass above the upper limit of B_2O_3, and therefore the limit of either PbO or B_2O_3 is exceeded in every number of the series. E–4, whose composition is above the limit in both PbO and B_2O_3, but not far above either, would naturally be selected as the nearest approach of any to a favorable combination.

An inspection of this series shows that the actual results are exactly what we would expect, and are therefore entirely consistent with the preceding series. E–1, E–2 and E–3 resemble C–5 and C–4 which are also high in lead, while E–5 and E–6 are almost identical with D–5 and D–6, containing respectively the same equivalents of B_2O_3. E–4 is not so good as D–4, owing to its lead content. This makes the series consistent throughout.

We may therefore conclude that in glazes of the types employed in these tests:

1. A high lead content destroys the color of the chromium-tin pink.

2. A high content of boracic acid is also detrimental to the pink color, and boracic acid in any quantity gives the color a purple tint.

3. The allowable limit of PbO is below .4 equivalents.

4. The limit of B_2O_3 is below 6. equivalents.

In view of these results, it is apparent that with a glaze of the type employed up to this point, a lead glaze without boracic acid will not answer the purpose: for not only does

a high lead content destroy the pink color, but it also produces a glaze which beads up and will not lie smooth, and the higher the lead content is made, the worse the glaze behaves. Moreover, with a leadless glaze, while it is possible to produce the desired effect at cone 3 or 4 by the use of boracic acid, we find that when sufficient boracic acid is used to mature the glaze at cone 01, the color effect is destroyed. Neither lead nor boracic acid alone is able to soften the glaze—

$$RO,\ 0.2\ Al_2O_3,\ 2.00\ SiO_2 \left\{ \begin{matrix} 0.005\ Cr_2O_3 \\ 0.144\ SnO_2 \end{matrix} \right\}$$

enough to make it mature at cone 01 without being detrimental to the color. Nevertheless, since the effects produced by the lead and boracic acids were opposite in character, it was thought that as much or more of each could be used with the other as without it, combining their softening effects, and neutralizing the detrimental tendencies of each other.

It was therefore decided to make another series, in which both the lead and boracic acid should be regularly increased, in order to determine to what extent each could be used in connection with the other. But before making these tests, it was considered desirable to determine whether it would be feasible to reduce the refractoriness of the glaze by decreasing the ratio of CaO to KNaO, or by decreasing the acidity of the glaze. It was also decided to reduce the quantity of pink fritt in the glaze, as this is one of the refractory agents.

The effect of decreasing the lime-soda ratio was tried by the use of the following series:

SERIES F.

No. of Glaze.	KNaO	CaO	Al_2O_3	SiO_2	Cr_2O_3	SnO_2	Ratio (CaO:KNaO)
1	.333	.666	0.2	2.00	0.003	0.087	2.00:1
2.	.400	.600	"	"	"	"	1.50:1
3	.500	.500	"	"	"	"	1.00:1
4.	.600	.400	"	"	"	"	0.66:1
5.	.666	.333	"	"	"	"	0.51:1

In making F-1, the following fritt was used:

$$\left.\begin{matrix}0.33\ KNaO \\ 0.67\ CaO\end{matrix}\right\} 0.13\ Al_2O_3,\ 1.00\ SiO_2$$

The raw materials added to this fritt to make the glaze were:

Spar	.04	Equivalents
Clay	.047	"
Flint	.698	"

The glaze batch contained over 50 per cent of the lime-soda fritt.

For F-5, a fritt was designed as follows, leaving only .07 equivalents of raw clay to add to the glaze batch:

$$\left.\begin{matrix}0.726\ KNaO \\ 0.24\ CaO\end{matrix}\right\} 0.143\ Al_2O_3,\ 1.94\ SiO_2$$

As a consequence, some allowances should be made in considering these results, for the fact that toward the latter end of this series, a greater proportion of the glaze mixture is fritted and that this tends to reduce the refractoriness somewhat. This difference was probably not great enough to be of much consequence, however.

These glazes were fired in the up-draft test kiln, cone 5 being reached in about fourteen hours. The resulting glazes showed the following effects:

As the ratio of CaO to KNaO decreased, the fusibility increased somewhat, but F-5 was the only one which was fully mature. However, with the increase of fusibility there was an increasing tendency for the glazes to bubble even though immature. The last ones of the series were rough at the edges, while smooth over the surface of the tile. As to color, the detrimental effects of the lower lime-soda ratio was not so great as had been anticipated, yet the color degenerated steadily from a bright pink in F-1 toward brown in F-5.

After making this test, it was decided to continue the use of the hightest ratio — CaO: KNaO=2:1, because the softening effect of the lower ratio was not considered great enough to compensate for the detrimental effect on the color.

Having fixed on a lime-soda ratio, another series was made to determine whether a reduction in acidity would

produce a less refractory glaze. In order that all the conditions except the acid ratio should remain constant in this series, all the ingredients of both extremes were fritted. The series was as follows:

SERIES G.

No. of Glaze	KNaO	CaO	Al_2O_3	SiO_2	Cr_2O_3	SnO_2	Acid Ratio
1.	0.333	0.666	0.200	2.00	.003	.087	2.50
2.	"	"	0.170	1.70	"	"	2.25
3.	"	"	0.143	1.43	"	"	2.00
4.	"	"	0.118	1.18	"	"	1.75
5.	"	"	0.097	0.97	"	"	1.50

This series, like series F, was fired to cone 5 in about fourteen hours. The result was not at all in favor of a lower ratio than 2.5. This glaze, G-1, which is the same as C-1, D-1, and F-1 was slightly immature, as was expected, but of fairly good color. However, as the ratio decreased, no good glazes were produced, all being immature, rough and badly blistered. The color, also, became poorer. It was decided, therefore, that for lime-soda glaze at least the acidity was none too high, and the type glaze RO, .2 Al_2O_3 2 SiO_2 was retained for the remaining series.

As has been stated, the following set of series was designed to demonstrate the combined effects of lead and boracic acid when used together. In each series the lead content was kept constant, while the B_2O_3 was regularly increased by increments of .1 equivalent from nothing up to .5 equivalent, beyond which it was judged unnecessary to go.

The lead content of the first series was placed at .15 equivalent, and each successive series contained an addition of .10 equivalent, making the lead content of the fourth series .45 equivalent, which was supposed to be above the limit.

The series were as follows:

SERIES H.

No.	PbO	CaO	KNaO	Al_2O_3	SiO_2	B_2O_3	Cr_2O_3	SnO_2
1	0.15	0.566	0.283	0.2	2.0	0.0	0.003	0.087
2	"	"	"	"	1.9	0.1	"	"
3	"	"	"	"	1.8	0.2	"	"
4	"	"	"	"	1.7	0.3	"	"
5	"	"	"	"	1.6	0.4	"	"
6	"	"	"	"	1.5	0.5	"	"

SERIES I.

No.	PbO	CaO	KNaO	Al_2O_3	SiO_2	B_2O_3	Cr_2O_3	SnO_2
1	0.25	0.50	0.25	0.2	2.0	0.0	0.003	0.087
2	"	"	"	"	1.9	0.1	"	"
3	"	"	"	"	1.8	0.2	"	"
4	"	"	"	"	1.7	0.3	"	"
5	"	"	"	"	1.6	0.4	"	"
6	"	"	"	"	1.5	0.5	"	"

SERIES J.

No.	PbO	CaO	KNaO	Al_2O_3	SiO_2	B_2O_3	Cr_2O_3	SnO_2
1	0.35	0.433	0.216	0.2	2.0	0.0	0.003	0.087
2	"	"	"	"	1.9	0.1	"	"
3	"	"	"	"	1.8	0.2	"	"
4	"	"	"	"	1.7	0.3	"	"
5	"	"	"	"	1.6	0.4	"	"
6	"	"	"	"	1.5	0.5	"	"

SERIES K.

No.	PbO	CaO	KNaO	Al_2O_3	SiO_2	B_2O_3	Cr_2O_3	SnO_2
1	0.45	0.366	0.183	0.2	2.0	0.0	0.003	0.087
2	"	"	"	"	1.9	0.1	"	"
3	"	"	"	"	1.8	0.2	"	"
4	"	"	"	"	1.7	0.3	"	"
5	"	"	"	"	1.6	0.4	"	"

The glazes of these series were made by blending the three following glazes:

1. A raw lead glaze nearly as high in lead as the lime and alkalies, introduced by the pink fritt would permit:

$$\left.\begin{matrix}0.800\ PbO\\0.133\ CaO\\0.066\ KNaO\end{matrix}\right\}\ 0.2\ Al_2O_3,\ 2.0\ SiO_2\ \left\{\begin{matrix}0.003\ Cr_2O_3\\0.087\ SnO_2\end{matrix}\right.$$

2. A fritted boracic acid glaze:

$$\left.\begin{matrix}0.666\ CaO\\0.333\ KNaO\end{matrix}\right\}0.2\ Al_2O_3\left\{\begin{matrix}1.0\ SiO_2\\1.0\ B_2O_3\end{matrix}\right.\left\{\begin{matrix}0.003\ Cr_2O_3\\0.087\ SnO_2\end{matrix}\right.$$

This glaze was made without any raw ingredients, using the pink fritt and one of the following composition:

$$\left.\begin{matrix}0.641\ CaO\\0.359\ KNaO\end{matrix}\right\}0.2\ Al_2O_3\left\{\begin{matrix}0.991\ SiO_2\\1.100\ B_2O_3\end{matrix}\right.$$

In all the Series except A and B, the pink fritt given for Series C has been used.

3. A fritted lime-soda glaze, made exactly as described for F-1, as follows:

$$\left.\begin{matrix}0.666\ CaO\\0.333\ KNaO\end{matrix}\right\}0.2\ Al_2O_3,\ 2.00\ SiO_2\left\{\begin{matrix}0.008\ Cr_2O_3\\0.087\ SnO_2\end{matrix}\right.$$

With these three glazes it was possible to make blends covering all combinations within the limits assigned. The calculations were made as follows: Suppose I-4 were to be made. This glaze contains .25 equivalent PbO and 0.3 B_2O_3. The lead extreme contains .8 equivalents PbO; therefore, we must use $0.25 \div 0.80 = .312$ equivalents of the lead glaze. For the B_2O_3 we take $0.3 \div 1 = .3$ equivalent of the boracic acid glaze. This make $0.312 + 0.3 = .612$ equivalent, leaving $1.000 - 0.612 = .388$ equivalent of the lime-soda glaze. Knowing the equivalent weight of each glaze, we have but to multiply each decimal by the corresponding equivalent weight for the batch weight.

These glazes were placed on end in saggers, which were then luted tight and placed in the down draft test kiln. The kiln was burned in about fourteen hours, cone 1 being reached. The results can be stated best by describing the individual pieces:

Series H. PbO=0.15.

H-1. Immature, rather pale pink with a margin of white enamel.

H-2. Glaze slightly darker and more nearly mature than H-1.

H-3. Glaze fused enough to develop a strong dark color, but not enough to run smooth. Scummed.

H-4. Glaze mature and color well developed but shows a tendency to segregate. Has run too much, leaving the color too thin near the top.

H–5 and 6. As the B_2O_3 is raised the tendency to flow increases, leaving the color too thin on the tile. The glazes tend toward greenish, but segregations of pink remain.

Series I. PbO=0.25.

I–1. Glaze beaded and immature, color inferior. White margin.

I–2. Still beaded but somewhat smoother and better color.

I–3. Glaze fairly smooth and of good color. Has run a little, leaving color rather thin and uneven near top. Scummed slightly.

I–4. Glaze mature and brilliant. Has run freely and the color is not dense enough near the top in consequence.

I–5 and 6. Pale glazes mottled with pink segregations. Pronounced green tint in No. 6.

Series J. PbO=0.35.

J–1. Badly beaded, poor color, white margin. Same tendencies as in I–1, but carried to a greater extent.

J–2. Beaded, most of the tile bare, but color fairly well developed.

J–3. Glaze nearly mature, color fair except at margin. Glaze has not run.

J–4. Mature. Has run considerably, small portions of the tile bare, color fair but not well distributed—similar to H–5 and I–5.

J–5. About like I–6.

J–6. Greenish glaze, mottled with pink.

Series K. PbO=0.45.

K–1. Beaded worst of all but the color not destoyed.

K–2. Still bad.

K–3. Not yet smooth, some spots bare, others thick. Pale margin at top.

K–4. Glaze mature and fairly smooth. Has run considerably. Color uneven and inferior.

K–5. Pale green glaze with only a faint trace of pink.

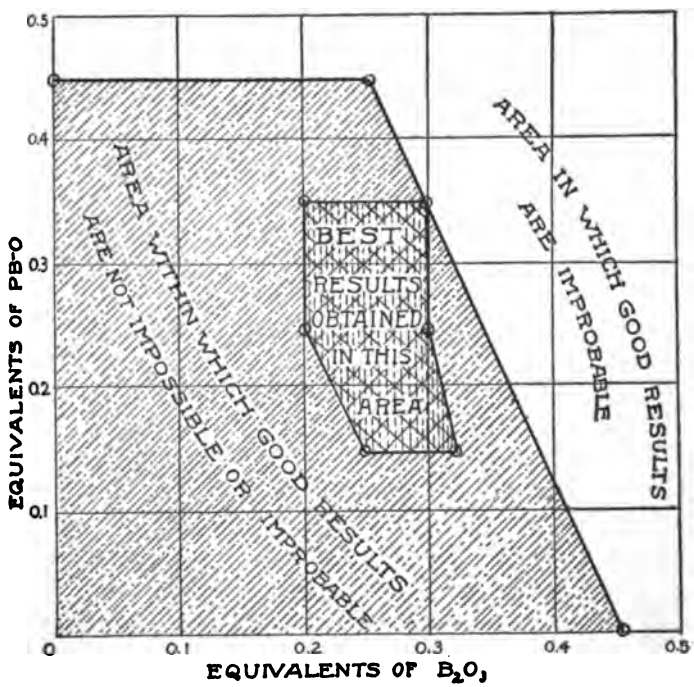
A review of these results brings out the following points:

1. In producing a chromium-tin pink glaze at low temperature, it is best to use both lead and boracic acid.

2. The more lead the glaze contains, the more boracic acid is required to overcome the tendency to bead; but the greater the lead content in the glaze, the less boracic acid can be used without destroying the color. This places a limit on the allowable lead content. These results indicate that this limit is below .45 equivalent of PbO.

3. Although more boracic acid can be used in glazes low in lead than in those with a high lead content, yet, even in leadless glazes, too much boracic acids destroys the pink color. As stated before, this limit is below 0.6 equivalent B_2O_3—probably below 0.5 B_2O_3.

The following diagram roughly defines the limits of PbO and B_2O_3 when used together for this purpose in a glaze of this type; all those combinations within the curve may be considered permissible so far as color is concerned, and may be expected to mature at some temperature. The most favorable combinations for cone .01 to cone 1 are within the cross-hatched area.



THE BEHAVIOR OF THE DIFFERENT PINK FRITTS IN THE SAME GLAZE.

These series of pink fritts have already been described. To these was added a fourth series, showing the effect of lime in the fritt.

SERIES A-IV.

No.	CaO	SnO_2	SiO_2	Cr_2O_3
19	0.00	1.00	2.00	0.0375
20	0.25	"	"	"
21	0.50	"	"	"
22	0.75	"	"	"
23	1.00	"	"	"
24	1.50	"	"	"
25	2.00	"	"	"
26	3.00	"	"	"

These fritts were prepared in a manner similar to that described for the previous fritt series. The appearance of the fritts was as follows:

19. Pale and inferior color with a greenish tint.

20. Somewhat stronger pink.

21. The color improves up to 22 with 0.75 CaO. From 22 upward, the color runs steadily toward brown, and the powder shows an increasing tendency to vitrify.

A glaze was now selected in which the different fritts were to be incorporated, in order to demonstrate the coloring effect of each in the glaze. The glaze used for the purpose was the following:

$$\left.\begin{matrix}0.25\ PbO\\0.50\ CaO\\0.25\ KNaO\end{matrix}\right\}\ 0.2\ Al_2O_3\ \left\{\begin{matrix}1.75\ SiO_2\\0.25\ B_2O_3\end{matrix}\right.$$

This glaze was prepared, and a portion of slip equivalent to forty five grams of dry glaze was weighed out for each fritt. Five grams of each fritt to be tested were then weighed out and each portion ground wet in an agate mortar and screened through a 200 mesh sieve into the receptacle containing the corresponding portion of glaze slip. The glaze was then stirred thoroughly to distribute the fritt through it. These glazes were then dipped, set and burned to cone 1, as described for the last set of series preceding.

The results may be described as follows:

The fritts of Series A-I, made from chromic acid and stannic oxide alone did not color the glaze well. No. 2 produced purple specks in the glaze, but poorly disseminated. In No. 4 the color was nearly destroyed, indicating that the proportion of chromic oxide was too high.

Of fritt Series A-II, containing 1 SnO_2, 0.0375 Cr_2O_3 and CaO varying, No. 10 with .391 CaO and No. 11 with 1.04 CaO were tried. Both gave good strong colors. The glaze of No. 10 was slightly scummed, while that of No. 11 was scummed badly and was less mature.

In Series A-III No. 15 produced a color not dissimilar to that produced by the Seger fritt, but somewhat lighter and perhaps a little better distributed. In 16 and 17 the color becomes weaker, owing to dilution of the fritt by silica.

The fritts of Series A-IV all produced fairly good colors. The glazes containing the fritts highest in lime, scummed worst, probably due to the fact that the higher lime content made the glazes more refractory.

The Seger fritt and my modification of the Seger fritt were also tried, and the effects were very similar to most of the others described.

These results indicate that a wide range in variation in the quantity of lime and silica used in the fritt is allowable, and that the color of the fritt is not a sure indication of the color which it will produce in a glaze. In this set of tests the best colors were produced by the fritts which contained no silica, but this was probably due to the fact that five grams of the flintless fritt represented a larger quantity of the other ingredients, and therefore had a greater coloring power. It seems that almost any combination of tin with a very small proportion of chromium, together with some lime or silica will make a fritt which is capable of producing a good pink color in glazes of the type—RO, 0.2 Al_2O_3, 2 SiO_2, provided that the ratio of lime to the alkalies in the glaze be kept high and the content of lead and boracic acid be kept within the limits assigned. It is not known to what chemical action the production of the pink color is due, but it is evident that while a high temperature is necessary for the

development of a fritt which will produce the pink color in a glaze, yet the action of the glaze at a comparatively low temperature, has the effect of changing the color of the fritt. The content of lime and the content of silica both have a marked effect on the color of the fritt, and yet a fritt containing silica and no lime, produces in the glaze a color similar to that developed by a fritt containing lime and no silica.

On the other hand, as has been demonstrated, a glaze too low in either lime or silica, is detrimental to the color. These observations lead one to emphasize the statement of Mr. Mayer, that in producing the different variations of this color, the greater part of one's attention should be paid to the composition of the glaze, and not to that of the pink fritt.

THE FLUXING POWER OF MICA IN CERAMIC BODIES.

BY

RAY THOMAS STULL, E. M., TERRA COTTA, ILL.

All clays contain mica, which is generally present in small amounts, but in some cases it comprises a considerable portion of the mass, sometimes reaching as high as 30 per cent.

The real effect of mica upon the refractory properties of a clay is not definitely known. At the present time but very little can be found in ceramic literature pertaining to the effect of mica in ceramic mixtures. Langenbeck, in his book entitled "The Chemistry of Pottery," speaks of mica as a flux, but gives no evidence as proof of this statement. "Technology of Ceramics," by Auscher et Quillard, says that mica should impart resistance and fusibility to a clay. What is here meant by resistance is rather vague, but probably refers to the strength of the body. The authors appear to be somewhat uncertain in regard to this statement, by using the word "should."

Ceramists hold that the fluxing value of feldspar is due to its alkali content. If the alkali is the real fluxing agent, then it seems reasonable to suppose that any other alkali bearing mineral would be equally good as a flux, in proportion to its alkali content. Others are of the opinion that a molecule of the fluxing mineral acts as a unit until the temperature is reached, at which fusion or decomposition of the mineral begins to occur. This fusion or decomposition may be brought about at a lower temperature by the action of some other substance upon it. This is generally the case of a flux acting upon a flux. In this way a high fire flux may be made to exert its fluxing power at a temperature far below that at which it would otherwise occur.

The white micas are divided into two groups, the alkali micas and the magnesian micas. The alkali micas form a group of minerals whose formulæ run somewhat parallel to those of the feldspars; the principal difference is that these

micas are generally higher in alumina. The close similarity in chemical construction of muscovite mica with that of orthoclase feldspar is, perhaps, one reason for their close association in nature. This close association accounts for the fact that the mica found in white clays is almost invariably the muscovite mica.

The formulæ given for orthoclase feldspar and muscovite mica are:

Orthoclase feldspar, $K_2O, Al_2O_3, 6\ SiO_2$
Muscovite mica, $K_2O, 3\ Al_2O_3, 6\ SiO_2, 2\ H_2O$

The similarities in composition are:

1. Each contains K_2O, Al_2O_3 and SiO_2.
2. Each contains one molecule of K_2O.
3. Each contains six molecules of SiO_2.

The dissimilarities in composition are:

1. Muscovite has $3\ Al_2O_3$ against $1\ Al_2O_3$ in orthoclase.
2. Muscovite has $2\ H_2O$ against none in orthoclase.

The ratio of K_2O to SiO_2 in each is 1 to 6. In so far as the K_2O is concerned, each mineral would have the same fluxing advantage. In orthoclase, the ratio of Al_2O_3 to SiO_2 is 1 to 6, while in muscovite the ratio is 3 to 6.

Seger found that by starting with pure alumina and making increased additions of silica, he could reduce the refractory qualities of the mixture up to a molecular ratio of Al_2O_3 to $SiO_2 = 1:17$, which gave him the most fusible combination of alumina and silica. On further additions of silica, the refractoriness of the mixture began to increase.

From this, the natural conclusion would be that the ratio, Al_2O_3 to $SiO_2 = 3$ to 6 in muscovite is more refractory than 1 to 6 in orthoclase. But, however, we cannot depend upon the theoretical assumption that the ratios of RO to SiO_2 and Al_2O_3 to SiO_2 determine the refractoriness, fluxing power or fusibility of a mineral when incorporated as a ceramic mixture, since the presence of other ingredients may materially change these qualities.

It was the object of the following work to determine, if possible, whether muscovite mica acted as a flux or whether it imparted refractory properties to a ceramic body, when fired at temperatures within the range for burning white

biscuit ware. Also, if it did have a fluxing power, how would it compare to that of orthoclase feldspar under the same conditions?

THE MATERIALS AND THEIR PREPARATION.

The materials used in this experiment were: 1 muscovite mica; 2 Brandywine feldspar; 3 Florida ball clay; 4 Golding flint.

The mica was the scrap variety from Jackson County, N. C. When received it ran in sizes from three inches across by one-half inch thick down to very small, thin flakes. In the laminae of some of the pieces were stains of iron. When observed, such pieces were rejected.

The mica was placed in a ball mill twenty-four inches in diameter by twenty-five inches in length. Three hundred and fifty pounds of Iceland flint pebbles were used, and sufficient water added to cover the balls three inches deep. The mica was allowed to grind for fifteen hours at thirty-six revolutions per minute. Owing to the tough nature of the mica, this long period of grinding was resorted to in order to reduce it as nearly as possible to the same degree of fineness as that of the feldspar, so that the two minerals should have the same advantages in fineness of grain. After grinding, the mica was removed from the mill, and screened through a laun of 120 meshes to the linear inch. That portion which remained on the laun was rejected, and represented less than 10 per cent. of the original batch. The portion which passed through the laun was placed in a tall can, allowed to settle over night, the supernatant liquid siphoned off, and the mica pulp evaporated to dryness.

The mica pulp when dry was nearly white in color, having a delicate light gray appearance. It was reduced to such a degree of fineness that the unaided eye could scarcely distinguish the fine mica flakes. On moistening with water it took on a dark gray appearance and developed a surprising degree of plasticity; in fact, it showed a greater degree of plasticity than some clays which are being worked successfully at the present time. This tends to prove that plasticity in kaolinite is due to fineness of grain and thin plate-like structure.

The commercially ground feldspar was taken from the bin, blunged thoroughly with water and screened through a 120 mesh laun in order to insure the proper degree of fineness, and to free it from woody fibre and other foreign material. But very little feldspathic residue was left on the laun. The portion passing through the laun was placed in a tall can, allowed to settle, decanted and dried in the same way as was done in the preparation of the mica.

The commercially prepared Florida ball clay, better known as Florida kaolin, was blunged to a thin slip and passed through the 200 mesh laun for the purpose of eliminating the foreign matter, and to take out as much of the mica as was possible to do. In this way over 0.5 per cent. of the weight of clay was removed as mica. The clay was allowed to settle, decanted and dried.

As the flint was known to be a very pure variety and of the proper degree of fineness, it was used from the bin without any extra preparation.

ANALYSES OF THE MATERIALS.

	Mica	Feldspar	Kaolin	Flint
SiO_2	66.14	66.89	45.69	99.23
Al_2O_3	17.57	19.13	39.28	....
Fe_2O_3	5.12	.19	.40	.51
CaO	1.20	.42	.90	....
MgO	.76	.12	.18	....
K_2O	5.31	12.01	.13	....
Na_2O	.99	1.88	.00	....
H_2O	3.02	.00	13.61	....
Total	100.11	100.09	100.19	99.74

The molecular formulæ calculated from these analyses, taking Al_2O_3 as unity, are:

MUSCOVITE, FORMULA NO. 1.

$$\left.\begin{matrix}.1243\ CaO\\ .1103\ MgO\\ .3280\ K_2O\\ .0929\ Na_2O\end{matrix}\right\}\ 1.0000\ Al_2O_3\ \left\{\begin{matrix}.1857\ Fe_2O_3\\ 6.399\ SiO_2\ (.974\ H_2O)\end{matrix}\right.$$

.6555 RO

Combining weight, 582.167

FELDSPAR, FORMULA NO. 2.

$$\left.\begin{matrix}.0400\ CaO\\ .0160\ MgO\\ .6812\ K_2O\\ .1574\ Na_2O\end{matrix}\right\} 1.0000\ Al_2O_3 \left\{\begin{matrix}.0063\ Fe_2O_3\end{matrix}\right. \left\{\begin{matrix}5.900\ SiO_2\end{matrix}\right.$$

.8946 RO — Combining weight, 538.679

FLORIDA KAOLIN, FORMULA NO. 3.

$$\left.\begin{matrix}.0417\ CaO\\ .0117\ MgO\\ .0036\ K_2O\end{matrix}\right\} 1.0000\ Al_2O_3 \left\{\begin{matrix}.0065\ Fe_2O_3\end{matrix}\right. \left\{\begin{matrix}1.977\ SiO_2\ (1.963\ H_2O)\end{matrix}\right.$$

.0560 RO — Combining weight, 260.167

In the mica formula it will be observed that the molecular ratio of K_2O to Al_2O_3 is approximately 1 to 3, which corresponds to the theoretical formula for muscovite. Attention is also called to the high percent of silica in the mica. This may be accounted for by the wear upon the flint pebbles during the long period of grinding. The "Chemistry of Geology," Volume II, by Bischoff, gives as the limits of composition for mica:

SiO_2	36 to 71
Al_2O_3	6 " 38
Fe_2O_3	0 " 36
MgO	0 " 29
K_2O	2 " 14

Undoubtedly some silica was introduced by the wear upon the flint pebbles, but nevertheless it is more than likely that the mica was the highly silicious variety.

MAKING THE BODIES.

Two body series were made, Series A and Series B. Series A consisted of eleven bodies, first starting with 100 percent Florida kaolin, decreasing the kaolin each time by 10 per cent. and adding a corresponding amount of ground mica, up to all mica and no kaolin. These bodies were marked A-1, A-2, A-3, etc., up to A-11. The composition of Series A by weight is:

A-1	100	Kaolin	0	Mica
A-2	90	"	10	"
A-3	80	"	20	"
A-4	70	"	30	"
A-5	60	"	40	"
A-6	50	"	50	"
A-7	40	"	60	"
A-8	30	"	70	"
A-9	20	"	80	"
A-10	10	"	90	"
A-11	0	"	100	"

These bodies were made for the purpose of determining whether mica had any fluxing action upon kaolin, or if it had any vitrifying properties alone.

If mica exerted a fluxing power, it was also desirable to know how this fluxing power compared with that of feldspar in an ordinary whiteware body, and to what extent it could take the place of feldspar in such a body, and its effect upon the general appearance of the body.

For this purpose Series B was made. This was done by blending two bodies, each one having the same chemical formula. The only difference in these two bodies was that the first one called "S" was made from flint, kaolin and feldspar, while the second, which was marked "M," was made from kaolin, flint and mica, without the use of feldspar.

As a starting point for making this series, a well known whiteware body was selected which would give a comparatively wide range of vitrification, shrinkage and porosity, within the temperature range for burning ordinary white biscuit ware. The batch weights for this body are:

Florida Kaolin.....	50%
Golding Flint.....	38
Brandywine Spar.....	12

Considering the flint as pure SiO_2, and using the formulæ for the kaolin and feldspar according to their ultimate analyses, the formula for this body figures:

FORMULA NO. 4.

$$\left.\begin{array}{l} .0415\ CaO \\ .0121\ MgO \\ .0758\ K_2O \\ .0165\ Na_2O \\ \hline .1459\ RO \end{array}\right\} \left.\begin{array}{l} 1.0000\ Al_2O_3 \\ .0065\ Fe_2O_3 \end{array}\right\{ 5.338\ SiO_2$$

Body "M" was made by taking the proper proportions of Florida kaolin and mica so as to furnish all the RO and Al_2O_3 in formula No. 4, and supplying the difference in SiO_2 by flint.

The calculation of Body M was as follows:

Let X = equivalent of kaolin
Let Y = " " mica

required to supply .1459 RO and 1.000 Al_2O_3. Then

.0574 X RO in kaolin + .6555 Y RO in mica = .1459 RO in formula 4
Also, X Al_2O_3 in kaolin + Y Al_2O_3 in mica = 1.000 Al_2O_3 in formula 4
X = .8514 equivalents of kaolin
and Y = .1486 " " mica

.8514 equivalents kaolin + .1486 equivalents mica give

$$.1459\ RO \left\{ \begin{matrix} 1.000\ Al_2O_3 \\ .033\ Fe_2O_3 \end{matrix} \right\} 2.623\ SiO_2$$

Then there remains 5.338—2.623 = 2.715 equivalents of flint to be added. The batch weights for body "M" are:

Kaolin	47.04%
Flint	34.59
Mica	18.37

Calculating the formula of this mixture as a check upon our operations, we find the following formula for body "M.',

FORMULA NO. 5.

$$\left.\begin{matrix} .0540\ CaO \\ .0263\ MgO \\ .0518\ K_2O \\ .0138\ Na_2O \end{matrix}\right\} \begin{matrix} 1.000\ Al_2O_3 \\ .033\ Fe_2O_3 \end{matrix} \Big\} 5.338\ SiO_2$$
$$.1459\ RO$$

In supplying the total RO and Al_2O_3 in formula No. 4 by kaolin and mica alone, the Fe_2O_3 has been overburdened by an amount .033—.0065 = .0265. This equivalent of Fe_2O_3 was added to formula No. 4, giving formula No. 6 as the formula for body "S."

FORMULA NO. 6.

$$\left.\begin{matrix} .0415\ CaO \\ .0121\ MgO \\ .0758\ K_2O \\ .0165\ Na_2O \end{matrix}\right\} \begin{matrix} 1.000\ Al_2O_3 \\ .033\ Fe_2O_3 \end{matrix} \Big\} 5.338\ SiO_2$$
$$.1459\ RO$$

In comparing formula No. 6 with that of No. 5, it is observed that the two have the same chemical equivalent of RO, and that they have the same Al_2O_3, Fe_2O_3 and SiO_2. In comparing the RO in No. 6 with that of No. 5, we see that No. 5 is higher in CaO and MgO, and correspondingly lower in K_2O and Na_2O, thus giving body "S" containing spar a slight advantage in fusibility over body "M" containing mica.

The batch weights for body "S" calculated from formula No. 6 are:

Kaolin	49.93%
Flint	37.81
Spar	12.16
Fe_2O_3	.10

Bodies "M" and "S" were each weighed from the dry materials, placed in separate porcelain lined ball mills and ground wet for thirty minutes. The two batches were then removed, screened through a 100 mesh screen, and placed in two separate cans. The percent of solid material per unit weight of slip was determined for each, and Series "B" was made by blending these two slips, as follows:

B-1	100	Body "S"	0	Body "M"
B-2	90	"	10	"
B-3	80	"	20	"
B-4	70	"	30	"
B-5	60	"	40	"
B-6	50	"	50	"
B-7	40	"	60	"
B-8	30	"	70	"
B-9	20	"	80	"
B-10	10	"	90	"
B-11	0	"	100	"

Each body of the series was thoroughly mixed by blunging, and dried on concave plaster slabs down to stiff mud consistency, for the purpose of making into brickettes.

MAKING THE TRIAL PIECES.

For each of the bodies in Series A and Series B, brickettes were made 3½ x 1 x ½ inches. Two pin points were made on each brickette three inches apart for the purpose of measuring shrinkage, and the brickettes dried carefully so as to prevent warping.

BURNING THE TRIALS.

Brickettes from each body of the two series were burned at two different temperatures, cone 4 and cone 9. For the cone 4 burn cones 2, 3, 4 and 5 were placed on a slab at the center of a large sagger, and the brickettes placed around the cones in close proximity to them. The sagger was luted tight, placed at the center of the small updraft kiln, and fired

with coke as fuel for a period of eight hours, when cone 5, which was placed outside of the sagger, went down. It was observed in previous burns that there was a difference in the temperature between the outside and inside of the sagger, of approximately one cone.

On taking the trials from the kiln, it was found that cone 4 was down so that its point touched the slab, and cone 5 had began to curl.

The cone 9 burn was originally intended for cone 8. The trials were set in the same way as in the cone 4 burn, cones 6, 7, 8 and 9 being placed in the center. Coke was the fuel used, and the firing period lasted ten hours. Cone 9 was fired flat outside of the sagger. On taking the trials from the kiln, it was found that cone 8 inside the sagger was nearly flat, and cone 9 was down so that the point touched.

APPEARANCE OF TRIALS.

Series A in the cone 4 burn showed a gradual darkening in color from the kaolin-white in body A-1, down to a dark brownish gray in body A-11. The cone 9 burn gave a gradation in color from the white in A-1 to a stoneware gray in A-11. The colors at cone 9 were much lighter than at cone 4; in fact the brown had entirely disappeared.

Brickettes containing 50 per cent. or more of mica were highly vitrified in the cone 9 burn, and had the appearance of being thinly glazed. Such trial pieces had approached the first stages of fusion, as they were filled with minute bubbles and had swelled considerably.

No difference in general appearance could be detected between the two extremes of Series B, the entire series being a light ivory yellow in color for the cone 4 burn. In the cone 9 burn the yellow had disappeared, giving a biscuit which was nearly white.

DETERMINING FIRE SHRINKAGE AND POROSITY.

Before placing the trials in the kiln each brickette was carefully measured, and after firing, measurements were again taken. The loss in length due to fire shrinkage, divided by the dry length before firing, gave the percent of fire shrinkage.

Porosity was determined by absorption. Each brickette was weighed in the dry, burned condition on the balances, soaked in distilled water for forty-eight hours, removed, the superficial moisture wiped off and again weighed. The difference between wet and dry weights gave the grams of water absorbed by the pores. The weight of water absorbed, divided by the weight of the brickette before absorption, gave a ratio for the comparison of porosity. The following table gives the fire shrinkage and porosity for each body of the two series:

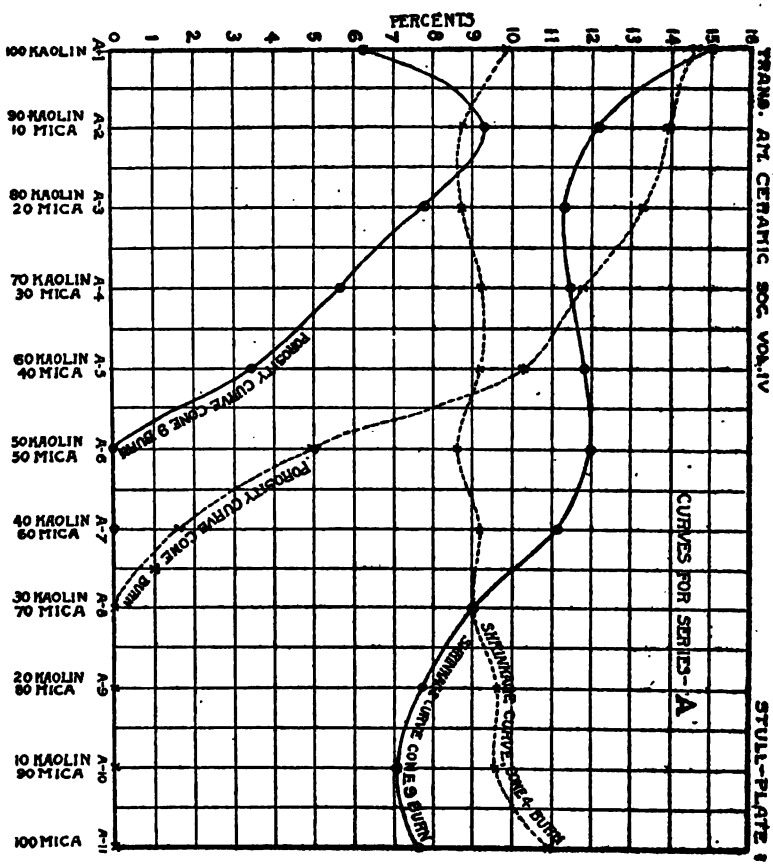
	Fire Shrinkage		Porosity	
	Cone 4	Cone 9	Cone 4	Cone 9
A- 1	9.87	15.13	14.58	6.34
A- 2	8.78	12.16	13.96	9.25
A- 3	8.78	11.41	13.40	7.88
A- 4	9.24	11.48	11.80	5.67
A- 5	9.21	11.84	10.24	3.43
A- 6	8.67	12.00	5.08	.00
A- 7	9.15	11.11	1.62	.00
A- 8	9.03	9.03	.05	.00
A- 9	9.68	7.80	.00	.00
A-10	9.61	7.01	.00	.00
A-11	10.90	7.60	.00	.00
B- 1	5.52	8.79	13.39	6.64
B- 2	4 97	7.69	14.13	6.81
B- 3	5.06	7.86	14.49	6.70
B- 4	4.47	7.18	15.23	7.74
B- 5	4.44	7.73	14.96	7.58
B- 6	3.87	7.14	16.13	7.97
B- 7	3.89	7.18	16.36	7.85
B- 8	3.91	7.22	16.70	8.22
B- 9	3.89	6.68	17.37	10.16
B-10	3.33	6.04	18.12	11.20
B-11	3.31	5.52	18.26	11.71

These results when plotted on curve sheets give some very interesting curves for study. On the following curve sheets the solid lines represent curves for the cone 9 burn, and the broken lines curves for the cone 4 burn.

STUDY OF THE CURVES.

In general, the shrinkage curve for the cone 4 burn shows a decrease in shrinkage from a content of 0 per cent. mica to 10 per cent. mica. From 10 per cent. to 70 per

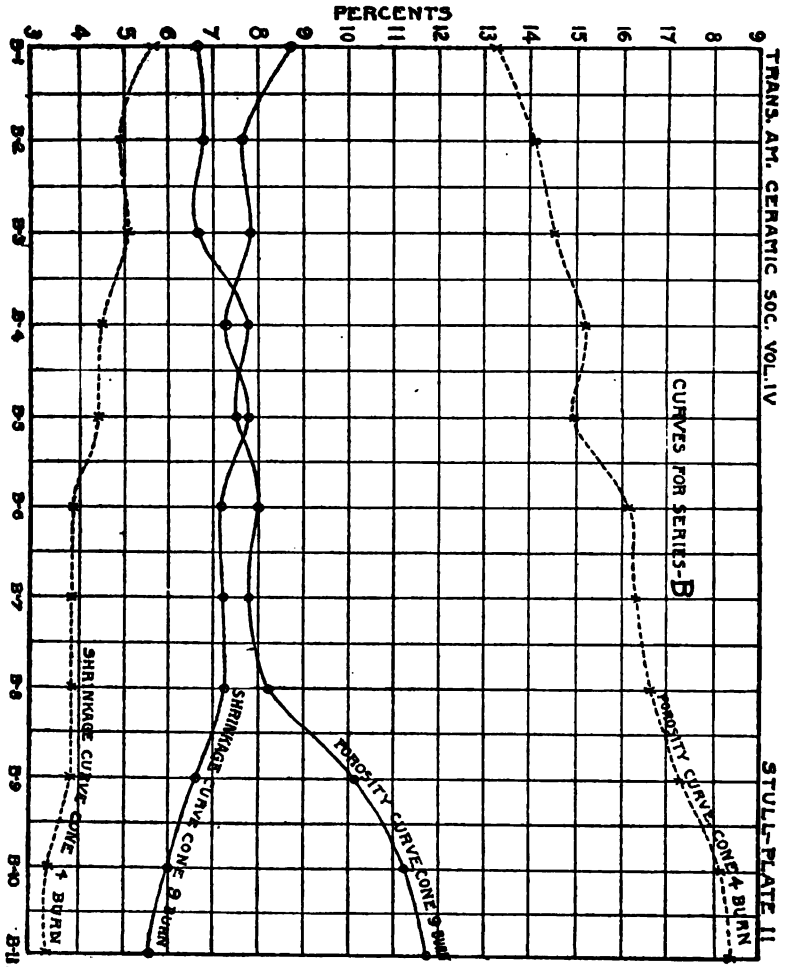
cent. mica the shrinkage is practically constant. From 70 per cent. to 100 per cent. mica there is an increase in shrinkage showing pure mica to have a shrinkage of 1 per cent. more than that of pure kaolin.



In the curve for the cone 9 burn there is a decrease in shrinkage from pure kaolin up to 20 per cent. mica. From 20 per cent. mica to 50 per cent. mica, the shrinkage increases. From 50 per cent. mica to pure mica the shrinkage drops.

Comparing the two shrinkage curves we see that they have a tendency to run parallel up to 50 per cent. mica. Beyond this point the tendeny is to run in opposite direc-

tions up to 90 per cent. mica. From 70 per cent. to 100 per cent. mica, the cone 4 burn has a greater shrinkage than the cone 9 burn. The difference in direction of the curve and in the decrease in shrinkage of the trials in the cone 9 burn, are accounted for by the swelling of the trial pieces when they contained from 50 per cent. to 100 per cent. mica. This was due to the vesicular structure produced in the advanced stage of vitrification or first stage of fusion.



The porosity curve for the cone 4 burn shows a continual decrease in porosity throughout, having no porosity

at 80 per cent. mica. In the cone 9 burn the curve shows an increase in porosity from 0 per cent. to 10 per cent. mica. From this point on porosity decreases, and when the content of mica reaches 50 per cent. porosity becomes zero. From a content of 10 per cent. mica, the two porosity curves run parallel.

With but one exception, the two porosity curves show that mica has exerted a fluxing action upon kaolin.

The one exception is a content of 10 per cent. mica in the cone 9 burn, where the porosity is greatest for that burn. This leads to the supposition that a content of 10 per cent. mica acts as a non-flux. This is also evidenced by the cone 4 and cone 9 shrinkage curves, which have their greatest falling off in shrinkage from pure kaolin to a content of 10 per cent. of mica.

The two shrinkage curves for Series B show a falling off in shrinkage from B–1 containing the feldspar to B–11 containing the mica. From B–6 to B–9 in the cone 4 burn, shrinkage remains constant. From B–6 to B–8 in the cone 9 burn, shrinkage also remains constant.

Both porosity curves for Series B show an increase in porosity from B .1 to B–11.

From the curves of Series B it is evident that mica has exerted a fluxing action, and that this fluxing action is less than that of feldspar.

SOURCES OF SHRINKAGE.

Fire shrinkage depends upon two factors, first, molecular contraction or condensation, and second, silicate formation produced by fluxing action. The latter shrinkage then depends upon the degree of vitrification brought about by the flux or fluxes present. When the flux has exerted no action, the body has had no secondary shrinkage—*i. e.*, this shrinkage is zero. When the flux has exerted its greatest power, then the body has its greatest shrinkage.

If the flux is present in sufficient quantities, the greatest shrinkage is at the point of the complete fusion. If the flux is present in small amounts, it may exert its full fluxing power long before fushion is reached, if no other flux is

present to continue the action. When the fluxing action has ceased, then the shrinkage depending upon it has ceased. Therefore, neglecting the shrinkage due to condensation the amount of which we do not know, it seems safe to say that the total shrinkage is roughly proportional to the fluxing power of the fluxes used. If we have two fluxes, which we will call C and D and place them under the same conditions and find that the body in which C was used gives a shrinkage of 5.52 per cent. and the body containing D gives 3.31 per cent. shrinkage, then

$$\frac{\text{Fluxing power C}}{\text{Fluxing power D}} = \frac{5.52}{3.31}$$

or fluxing power C is to fluxing power D=1 to 0.6. This is the case for Series B in the cone 4 burn. B–1 containing feldspar has a shrinkage of 5.52 per cent. and B–11 containing mica has 3.31 per cent. Since the difference in shrinkage due to molecular condensation is so small, we can take the above ratio as correct for all practical purposes.

B–1 contains 12.16 per cent. feldspar, and B–11 18.37 per cent. feldspar and B–11 18.37 per cent. mica by weight. Since the two bodies have the same general chemical constitution, we can say that the proportionate fluxing power of 12.16 pounds feldspar to 18.37 pounds mica=1 to 0.60 when placed under these conditions, and fired at the temperature of cone 4.

In the same way we find the proportionate fluxing power of 12.16 pounds feldspar to 18.37 pounds mica=1 to 0.63 when fired at the temperature of cone 9.

We see that these two proportions are practically the same. Multiplying the second and fourth members of the proportion by ten-sixths, gives the proportionate fluxing powers of 12.16 pounds spar to 30.61 pounds mica=1 to 1. Then 30.61 parts of mica are required in this case to produce the same fluxing action as 12.16 parts of feldspar, or 2.307 equivalents of mica produce the same fluxing power as one equivalent of feldspar, other things being equal.

CONCLUSIONS.

Drawing conclusions from the foregoing work, we have proven :

First. Mica, if fine enough, is plastic; hence, it need not necessarily cut down the plasticity of the clay.

Second. Mica, if fine enough, exerts a fluxing action upon kaolin and upon ceramic mixtures at temperatures below cone 4.

Third. Mica alone, when reduced to an extremely fine powder, vitrifies sufficiently to produce a non-absorbent body below the temperature of cone 4.

These results are contrary to the opinions expressed by the majority of ceramists. That mica is here proven to be a flux confirms the statement of Langenbeck and of Auscher et Quillard. Vogt, the eminent director of the National Porcelain Manufactory at Sevres, France, has shown that mica does not fuse at 1300 degrees C, but the condition under which the result was obtained is not known to the writer.

Mayer* and others have expressed their opinion in regard to the refractory qualities of mica by citing instances of finding mica flakes unchanged in the ware after passing the biscuit fire. These statements in regard to the refractory properties of mica, do not necessarily oppose the foregoing conclusions of this work. A substance to be an efficient and satisfactory body flux must be present in a fine state of division and disseminated uniformly throughout the mass. Substances present in coarse grains do not lose their identity, even when the mass is vitrified so as to be non-absorbent. Too much stress cannot be placed upon the advantages gained in fineness of grain. The finer the materials are prepared and the more intimately they are mixed, the nearer we get molecule to molecule, which materially aids both chemical action and fusion.

The mica used in this work was ground as fine as the feldspar, hence it had that same advantage of fusibility and chemical activity, in so far as fineness of the grain was concerned. Under these fair conditions we find that mica is unmistakably a flux, though not so good as feldspar.

*Vol. II, Trans. A. C. S., pp. 98, 99.

SOME GAPS IN OUR KNOWLEDGE OF CEMENTS.

(A NOTE.)

BY

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Notwithstanding the enormous development in the manufacture of cement, there yet remain many unanswered questions pertaining to its composition, its properties and peculiar reactions, some of which so far have baffled the most ingenious and refined attacks of research. It is the aim of this note to indicate a few of these problems to those who have not made a special study of this line.

Though apparently the composition of cements is a simple matter from the chemical standpoint, since it deals mainly only with the silicates and aluminates of lime and magnesia, we have at present no definite knowledge of the process of formation of these salts. We know in a general way that, with increasing temperatures, the basicity of the silicates and aluminates is increased, but exact information as to the proportions involved in these heat reactions is still lacking. Thus for instance, we are still at a loss to trace the connection between natural and Portland cements. This state of affairs is due to the fact that we are not yet in possession of data involving the examination of all the possible heat reactions with the help of accurate temperature measurements. It is absolutely essential to understand the fundamental principles involved in the formation of the simpler calcium compounds, before conclusions can be reached with reference to the complex combinations of Portland cement. If the elaborate experiments carried on by some of our able investigators were directed along this line, instead of dealing exclusively with such difficult problems as the determination of free lime in Portland cement, more rapid progress could perhaps be made. Synthesis certainly promises more direct results than analysis.

A series of synthetical compounds ranging, say, from 0.5 CaO (MgO) SiO_2 to 3.0 CaO (MgO) SiO_2, and from 0.5

$CaO\,(MgO)\,Al_2O_3$ to 2.0 $CaO\,(MgO)\,Al_2O_3$ burnt at different temperatures, exactly determined, is bound to produce results which will assist materially in clearing up the chemical laws governing the development of hydraulicity. In preparing these mixtures it is, of course, important that the materials be ground together as intimately as possible under exactly uniform conditions. Mere mixing is bound to be unsatisfactory. The progress of the combination with increasing temperatures can be followed by the following means:

1. Determination of the soluble silica (soluble in HCl and Na_2CO_3 solution) in the case of the lower lime compounds.

2. The heat of reaction of the resulting compounds as determined by the calorimeter.

3. The tensile strength of the cements as a measure of hydraulicity.

4. Determination of the water of hydration.

5. Boiling test.

6. Miscroscopic examination.

Perhaps the greatest confusion of opinion exists in regard to the relative hydraulic values of lime and magnesia and to illustrate the diversity of opinion existing on this topic, I shall quote somewhat at length as to various results obtained by a number of investigators.

In 1820, Fuchs, in working with several magnesia minerals such as talc, produced a good hydraulic cement. He found that magnesia burned at a lower temperature and was chemically more active than lime.

Heldt, another investigator, was not able to produce any hydraulic compounds from magnesia minerals and artificially prepared silicates.

Rivot again says, that magnesia behaves similarly to lime and forms with silica hydraulic compounds. He did not advise the use of magnesia cements, because the magnesia silicates and aluminates do not take up water as readily as the corresponding lime compounds.

Lieven, on the other hand, says that good cements cor-

responding to the formula $Mg\ SiO_3$ can be produced, which, when hydrated, possesses the formula $2(MgO\ SiO_2).9\ H_2O$.

Beginning in the year 1887, a series of experiments was carried out by the Society of German Portland Cement Manufacturers, in which Dyckerhoff showed that magnesia in Portland cement, that is, in a vitrified cement promotes excessive expansion, and causes the cement to lose its property of being constant in volume. This result was vigorously disputed by A. Meyers and others, who claimed that the bad results were due to other deficiencies in the character of the cements tested.

In 1893, Kawalewsky made an extensive report on vitrified magnesia cements, which after ten years were still found to be constant in volume and which had shown high strength.

Golinelli, in 1895, reported a most carefully arranged series of tests with magnesium Portland cements, containing an average of 26 per cent. of MgO and corresponding to the ratio:

$$\frac{CaO + MgO}{SiO_2 + Al_2O_3 + Fe_2O_3} = 2.06.$$

The silica-alumina ratio maintained was 3. These cements possessed most excellent strength, and were entirely constant in volume. This investigator came to the conclusion that magnesia may well be substituted for the lime.

Prof. Tetmajer, perhaps the most widely known authority on the testing of cements, on examining two magnesia cements, found that they possessed a strength equal to that of Portland cement, and that they were constant in volume as tested by the standard boiling tests.

Spencer B. and W. B. Newberry, in 1897, arrived at the conclusion that magnesia, though possessing marked hydraulic properties when heated alone, yields no hydraulic products when heated with silica and alumina, and probably plays no part in the formation of cement. It is incapable of replacing lime in cement mixtures, the composition of which should be calculated on the basis of lime without regard to the magnesia present.

This great diversity of opinion can only be explained by the assumption that all these investigators worked under different conditions, neglecting the factors of temperature, mineralogical composition, fineness of grain, etc.

Evidence shows that many of our best American natural cements, such as the Rosendale, containing an average of 17 per cent. of magnesium carbonate, are equal in hydraulic value to those natural cements in which lime alone is present. Vitrified natural magnesia cements have already been tested some forty years ago by General Q. A. Gilmore, and were found to be highly hydraulic. Yet, some data show that if magnesia is present in a vitrified cement in a certain ratio to the lime, it is liable to cause trouble in hardening, owing to the fact that magnesia hydrates much more slowly than lime. The crystallization thus brought about by the magnesia falls into a period in which the lime silicates have already assumed a certain rigidity, thus giving rise to a tendency of the cement to increase in volume.

This seems to take place only under certain conditions, and hence many magnesia cements do not give any trouble at all. In the investigation of this subject, the difference existing between the natural magnesia cements burnt at a lower temperature, and magnesia cements burnt to vitrification should be duly considered, as this difference is both chemical and physical.

In the investigation of Portland cements, it is extremely important to study clays which, as we know, enter into the composition of many of our cements, and which often give rise to serious trouble in practical work. Cement chemists, as a rule, are perhaps inclined to study clays from the standpoint of the ultimate analysis almost exclusively without paying so much attention to their mineralogical character. We should remember that there are methods for the mineral examination of clays, as the method of Dr. Bischof, Professor Seger and the modified Lunge-Schochor method, adopted by the Ohio Geological Survey.

All clays may be said to be principally composed of three minerals: the clay substance proper, giving rise to plasticity and possessing the ideal composition Al_2O_3,

$2\,SiO_2$, $2\,H_2O$; feldspathic minerals remaining from incomplete decomposition of the original feldspar, and having the general formula RO, Al_2O_3 6, SiO_2; and quartz in various degrees of fineness.

These three minerals show marked differences in regard to their reactions with lime when subjected to heat. Clay substance, represented in its purest condition by washed kaolin or china clay, and in various degrees of impurity by the ordinary brick clays, reacts most readily with lime and is decomposed at very low temperatures, giving rise at first to a mono-silicate, which at a high temperature becomes a bi- and finally a poly-silicate. The first stage is the replacement of the combined water of the clay by the lime, a reaction which is most energetic, as is shown by the fact that clay substance decomposes calcium sulphate much more readily than silica at a lower temperature, and under but slightly reducing kiln conditions. After the expulsion of the chemical water, the union with more and more lime is a function of temperature.

The feldspathic minerals, though they do not combine with lime as energetically as clay substance, are not far behind in chemical activity, and owing to their high content of combined silica, they are an ideal basis for a cement. Beside introducing a desirable silica-alumina ratio, the minerals of the feldspathic type by means of their alkali content, lower the vitrification point of cements, thus decreasing the difficulty of burning.

The quantity of alkalies brought in, even if pure feldspar were used as the basis of cement, does not reach the point where it becomes injurious to the cement, for it must be remembered that combined alkali has an entirely different function from alkali brought in merely in the form of a carbonate. What Prof. Newberry says in regard to the addition of free alkali does not apply to a silicate like feldspar.

In this connection the interesting question of the production of white Portland cement might be mentioned. It seems to be an entirely feasible plan to produce a high grade white Portland cement by means of a white clay, high in

feldspathic matter, such as the Mt. Holly clay of Pennsylvania. The same end can be attained by preparing an artificial mixture of kaolin, feldspar and flint, which, of course, would be more expensive than the use of a naturally occuring easily fusible material. Several instances have come to the writer's notice, in which the production of a white cement was attempted simply by the use of kaolin and a lime bearing material. Since such a mixture, owing to the infusible nature of the kaolin, is extremely difficult to clinker, beside resulting in a highly aluminous cement, it is not surprising that the experiments proved failures.

The quartz unites with lime most reluctantly and only when finely ground, as has been shown by the writer in previous work, and hence the entire raw grinding process must be directed towards grinding the free quartz so fine that it will combine in the kiln with the desired amount of lime; but this has its commercial draw back. Experiments designed to show how far the grinding must be carried on in a tube mill, so as to produce a good cement from clays containing certain amounts of more or less coarse quartz are urgently needed, and would prove very valuable. Also, it would be exceedingly instructive to know just what size of quartz is absolutely necessary for combination with the lime in the rotary kiln. By knowing this, it might be possible to decrease the lime content, allowing a certain excess of inert silica, and preventing the presence of free lime. The problem certainly has a decided commercial interest.

This also brings us to the question of using quartz introduced into the cement mixture in the shape of sand stone. There is no reason why an addition of quartz to highly aluminous clays should not be feasible and practicable, though in this case it would be necessary to use a set of separate crushing and grinding machinery, which would grind the sand stone to quite a fine powder before it is added to the clay and lime materials. Since the amount of quartz thus required is comparatively small, it would seem that the expense of the extra grinding is not prohibitive.

Some interesting data could be collected by studying the residue insoluble in HCl and Na_2CO_3 solution of various

Portland cements, made from different raw materials; this residue, of course, consists practically of inert silica and carbon, and is bound to tell a tale of the sufficiency of grinding and combination.

Beside the mineralogical consideration of the clays used in cement making, the general chemical question of the best ratio between silica and alumina is one which deserves thorough attention, and would amply repay the labor of determining it. All who are acquainted with the manufacture of Portland cement, realize that clays too high in alumina produce cements apt to show very disagreeable and dangerous properties.

In the United States about three general types of grinding machinery are in use for this grinding of the raw and burnt cement. It is the object of grinding the raw mixture to establish as intimate a molecular contact as practicable, thus bringing about as complete a chemical reaction as possible. On the other hand, the clinker must be ground as finely as possible in order that the highest cementing power may be produced, and it is well known that the higher the percentage of the very finest particles in a cement, the greater will be the strength and bonding power. The amount of this extremely fine powder can be roughly estimated by the use of sieves, but this kind of test does not offer a sufficiently accurate basis of comparison. We can, however, arrive at a satisfactory determination by means of a mechanical separation of various sizes of particles, arbitrarily fixed. The most convenient method is that of suspension, in which the powder is allowed to settle a given length of time in beakers containing water if the raw mixture is to be examined, and 95 per cent. alcohol if a cement is to be subjected to the test. The sizes thus obtained are measured quite readily by means of a micrometer slide attached to a microscope. In this manner the true efficiency of a grinding machine can be determined, for the apparatus producing the greatest amount of the finest particles per unit of power is the most desirable machine.

Finally, the writer would like to allude to a most important economic problem, namely, the determination of the

suitability of the various coals for the burning of Portland cement in the rotary kiln, a question involving elaborate experiments and a great outlay, but amply justified by the importance of the matter at issue. The question which coals are and which are not suitable for this purpose, comprises besides the strictly thermal questions of calorific power, calorific intensity, inflammability, also the investigation of the effects produced by the sulphur in the coal.

In conclusion, it might be said that a number of the investigations outlined are now being carried on by the Ohio Geological Survey, and are being pushed towards completion as rapidly as possible. It is hoped by the Survey that the work may contribute some definite knowledge to the store of information already available, and that the American manufacturers may be induced to adopt the liberal policy, so splendidly manifested by the Society of German Portland Cement Manufacturers, to whom the world is indebted for so much scientific information. This body of manufacturers, by contributing large sums of money for the establishment of a fine laboratory, and the carrying on of research work for the benefit of its members, as well as by doing independent individual work, has accomplished results unexcelled in the annals of any manufacturer's organization. There is no reason whatever why the American Portland cement manufacturers should not accomplish as much or more, by simply co-operating along these lines. The benefits thus reaped would be of such magnitude, that the slight expenditure of time and money necessary would be positively insignificant.

NOTES ON THE DEVELOPMENT OF GREENS FROM CUPRIC OXIDE IN GLAZES.

BY

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The notes presented in this paper do not cover the subject as fully as I had intended, as I found my time too limited to undertake a larger investigation and have it ready for this meeting.

The experiments have been confined to the following basic oxides; lime, potash, soda, lead, barium, zinc and magnesia, displacing each other in the formulæ in regular order, so that the changes occuring, and the effects upon cupric oxide could be readily noted.

Green glazes produced with copper are subject to so many changes when under fire, that it becomes very necessary to use every care, both in the making and burning of the glaze. The least variation of the temperature or the method of firing the kiln, will change the color, varying from a bright green to a brown or yellow shade.

The volatility of copper also causes considerable trouble; the fumes are absorbed by the sagger, and are liberated when again placed in the kiln, tinting the ware in the sagger; a white glaze showing quite strongly the effect by absorption of the green color. To avoid difficulties of this kind it becomes necessary to have separate saggers for colors containing copper oxide. Where it is desired to produce transparent glazes of a green color at ordinary kiln temperatures, it is necessary to use oxide of copper; the other mineral oxides produce greens which are more or less opaque.

For the purpose of ascertaining the action of the basic oxides, the change of color and the fluxing produced in combination with oxide of copper, and a correction, if possible, of some of these troubles, a line of experiments was decided upon as follows:

First. The effect of the different basic oxides in combination, and their action in developing the green color from a constant content of cupric oxide.

Second. Increase of alumina content, keeping the silica content as a mono-silicate or bi-oxygen ratio.

Third. Increase of silica content, with the alumina content remaining constant.

Fourth. Replacement of silica content with an equivalent proportion of boric acid, retaining the same acidity with an increase in oxygen ratio.

Fifth. Increase of the alumina content with silica content remaining constant, thus producing a reduction of the acidity.

Sixth. Development under same conditions of fire and temperature on a silicious dust-pressed body, and an earthenware plastic body.

Seventh. Experiments to be made at cone 02 and 2, showing the change, if any, caused by the increased temperature.

To lessen the work as much as possible, at the same time to retain close enough interchange of the bases as outlined in the first proposition, 0.1 equivalent of cupric oxide was decided to be sufficient copper strength to show the changes, leaving 0.9 equivalent to make up the RO elements for all of the experiments.

This was done by making a change of .15 equivalent in each combination, beginning, as you will note, by referring to experiments one to six, at .45 equivalents each of two elements, down to .15 equivalent each of six elements, as seen in Nos. 91 to 97; being a gradual replacement and interchange of the basic oxides, and final elimination of lime in No. 97. The Al_2O_3 and SiO_2 content was the same in all of these experiments.

In the series of experiments Nos. 98 to 137, in which there is an increase of alumina, retaining the bi-oxygen ratio; increase of silica; introduction of boric acid; an increase of alumina, keeping constant content of silica; the RO elements were kept the same, to enable an easy and just comparison with each series.

The following table of formulæ gives a complete view of the glazes under consideration:

No.	CuO	CaO	K_2O	Na_2O	PbO	BaO	ZnO	MgO	Al_2O_3	SiO_2	B_2O_3
1	.10	.45	.45	....	...	....	..	....	.20	1.60	....
2	.10	.45	...	.45	...	....	....	....	.20	1.60	....
3	.10	.45	...	....	.45	....	....	....	.20	1.60	....
4	.10	.45	...	....	...	.45	.. .	....	.20	1.60	....
5	.10	.45	...	....	...	....	.45	....	.20	1.60	....
6	.10	.45	...	....	...	....	....	.45	.20	1.60	. ..
7	.10	.45	.30	.15	...	....	....	....	.20	1 60	....
8	.10	.45	.30	....	.15	...	....	...	.20	1.60	....
9	.10	.45	.30	....	...	.15	....	....	.20	1.60	....
10	.10	.45	.30	....	...	....	.15	....	.20	1.60	....
11	.10	.45	.30	....	...	....	....	.15	.20	1.60	....
12	.10	.45	.15	.30	...	....	....	....	.20	1.60	....
13	.10	.45	...	.30	.15	....	....	....	.20	1.60	....
14	.10	.45	...	.30	...	.15	....	....	.20	1.60	....
15	.10	.45	...	.30	...	....	.15	....	.20	1.60	....
16	.10	.45	...	.30	...	....	....	.15	.20	1.60	....
17	.10	.45	...	.15	.30	....	....	....	.20	1.60	....
18	.10	.45	.15	....	.30	...	....	....	.20	1.60	....
19	.10	.45	...	....	.30	.15	....	....	.20	1.60	....
20	.10	.45	...	....	.30	....	.15	....	.20	1.60	....
21	.10	.45	...	....	.30	...	....	.15	.20	1.60	....
22	.10	.45	...	....	.15	.30	....	....	.20	1.60	....
23	.10	.45	...	.15	...	.30	....	....	.20	1.60	....
24	.10	.45	.15	....	...	.30	....	....	.20	1.60	....
25	.10	.45	...	....	...	.30	.15	....	.20	1.60	. ..
26	.10	.45	...	....	...	.30	....	.15	.20	1.60	....
27	.10	.45	...	....	...	.15	.30	....	.20	1.60	....
28	.10	.45	...	....	.15	....	.30	....	.20	1.60	....
29	.10	.45	...	.15	...	....	.30	....	.20	1.60	....
30	.10	.45	.15	....	...	....	.30	....	.20	1.60	....
31	.10	.45	...	....	...	....	.30	.15	.20	1.60	....
32	.10	.45	...	....	...	....	.15	.30	.20	1.60	....
33	.10	.45	...	....	...	.15	....	.30	.20	1.60	....
34	.10	.45	...	....	.15	....	....	.30	.20	1.60	....
35	.10	.45	...	.15	...	....	....	.30	.20	1.60	....
36	.10	.45	.15	....	...	....	....	.30	.20	1.60	....
37	.10	.30	.30	.30	...	....	....	....	.20	1.60	....
38	.10	.30	.30	....	.30	....	....	....	.20	1.60	....
39	.10	.30	.30	....	...	.30	.. .	....	.20	1.60	....
40	.10	.30	.30	....	...	....	.30	....	.20	1.60	....
41	.10	.30	.30	....	...	....	....	.30	.20	1.60	....

No.	CuO	CaO	K_2O	Na_2O	PbO	BaO	ZnO	MgO	Al_2O_3	SiO_2	B_2O_3
42	.10	.30	...	.30	.30	....	....	....	.20	1.60	....
43	.10	.30	...	.30	...	.30	....	....	.20	1.60	....
44	.10	.30	...	.30	...	....	.30	....	.20	1.60	...
45	.10	.30	...	.30	...	....	...	.30	.20	1.60	....
46	.10	.30	...	....	.30	.30	....	...	.20	1.60	....
47	.10	.30	...	....	.30	....	.30	....	.20	1.60	....
48	.10	.30	...	...	.30	....	....	.30	.20	1.60	....
49	.10	.30	...	....	...	.30	.30	....	.20	1.60	....
50	.10	.30	...	....	...	.30	....	.30	.20	1.60	....
51	.10	.30	...	....	...	....	.30	.30	.20	1.60	....
52	.10	.30	.30	.15	.15	....	...	....	.20	1.60	...
53	.10	.30	.30	.15	...	.15	....	....	.20	1.60	....
54	.10	.30	.30	.15	...	....	.15	....	.20	1.60	...
55	.10	.30	.30	.15	...	....	....	.15	.20	1.60	....
56	.10	.30	.30	....	.15	.15	....	....	.20	1.60	....
57	.10	.30	.30	....	.15	....	.15	....	.20	1.60	....
58	.10	.30	.30	....	.15	....	....	.15	.20	1.60	....
59	.10	.30	.30	....	...	.15	.15	....	.20	1.60	....
60	.10	.30	.30	....	...	.15	....	.15	.20	1.60	....
61	.10	.30	.30	....	...	....	.15	.15	.20	1.60	....
62	.10	.30	...	.30	.15	.15	....	....	.20	1.60	....
63	.10	.30	...	.30	.15	....	.15	....	.20	1.60	....
64	.10	.30	...	.30	.15	....	....	.15	.20	1.60	...
65	.10	.30	.15	.30	.15	....	....	....	.20	1.60	....
66	.10	.30	...	.30	...	.15	.15	....	.20	1.60	....
67	.10	.30	...	.30	...	.15	....	.15	.20	1.60	....
68	.10	.30	.15	.30	...	.15	....	....	.20	1.60	...
69	.10	.30	...	.30	...	....	.15	.15	.20	1.60	....
70	.10	.30	.15	.30	...	....	.15	....	.20	1.60	....
71	.10	.30	.15	.30	...	...	....	.15	.20	1.60	....
72	.10	.30	...	....	.30	.15	.15	....	.20	1.60	....
73	.10	.30	...	....	.30	.15	....	.15	.20	1.60	....
74	.10	.30	...	.15	.30	.15	....	....	.20	1.60	....
75	.10	.30	.15	....	.30	.15	....	....	.26	1.60	....

No.	CuO	CaO	K_2O	Na_2O	PbO	BaO	ZnO	MgO	Al_2O_3	SiO_2	B_2O_3
76	.10	.30	...	....	.30	....	.15	.15	.20	1.60	....
77	.10	.30	...	.15	.30	....	.15	....	.20	1.60	....
78	.10	.30	.15	....	.30	....	.15	....	.20	1·60	....
78-A	.10	.30	...	.15	.30	...	....	.15	.20	1.60	....
78-B	.10	.30	.15	....	.30	....	...	.15	.20	1.60	....
79	.10	.30	...	....	...	.30	.15	.15	.20	1.60	....
80	.10	.30	...	....	.15	.30	.15	....	.20	1.60	....
81	.10	.30	...	.15	...	.30	.15	....	.20	1.60	....
82	.10	.30	.15	....	...	.30	.15	....	.20	1.60	....
82-A	.10	.30	...	....	.15	.30	....	.15	.20	1.60	....
82-B	.10	.30	...	.15	...	.30	....	.15	.20	1.60	....
82-C	.10	.30	.15	....	...	.30	....	.15	.20	1.60	....
83	.10	.30	...	....	...	.15	.30	.15	.20	1.60	....
84	.10	.30	...	....	.15	....	.30	.15	.20	1.60	....
85	.10	.30	...	.15	...	....	.30	.15	.20	1.60	....
86	.10	.30	.15	....	...	....	.30	.15	.20	1.60	....
87	.10	.30	...	...	...	.15	.15	.30	.20	1.60	....
88	.10	.30	...	....	.15	....	.15	.30	.20	1.60	....
89	.10	.30	...	.15	...	....	.15	.30	.20	1.60	....
90	.10	.30	.15	....	...	....	.15	.30	.20	1.60	....
91	.10	.15	...	.15	.15	.15	.15	.15	.20	1.60	....
92	.10	.15	.15	....	.15	.15	.15	.15	.20	1.60	....
93	.10	.15	.15	.15	...	.15	.15	.15	.20	1.60	....
94	.10	.15	.15	.15	.15	....	.15	.15	.20	1.60	....
95	.10	.15	.15	.15	.15	.15	....	.15	.20	1.60	....
96	.10	.15	.15	.15	.15	.15	.15	....	.20	1.60	....
97	.10	...	.15	.15	.15	.15	.15	.15	.20	1.60	....
98	.10	.30	.15	....	.30	....	.15	....	...	1.00	....
99	.10	.30	.15	....	.30	....	.15	....	.05	1.15	....
100	.10	.30	.15	....	.30	....	.15	....	.10	1.30	....
101	.10	.30	.15	....	.30	....	.15	....	.15	1.45	....
102	.10	.30	.15	...	.30	...	.15	....	.20	1.60	....
103	.10	.30	.15	....	.30	....	.15	....	.25	1.75	....
104	.10	.30	.15	....	.30	....	.15	....	.30	1.90	....
105	.10	.30	.15	....	.30	....	.15	....	.35	2.05	....
106	.10	.30	.15	...	.30	....	.15	....	.40	2.20	....
107	.10	.30	.15	...	.30	....	.15	....	.20	1.00	....
108	.10	.30	.15	....	.30	....	.15	....	.20	1.25	....
109	.10	.30	.15	....	.30	....	.15	....	.20	1.50	....
110	.10	.30	.15	....	.30	....	.15	....	.20	1.75	....
111	.10	.30	.15	....	.30	...	.15	....	.20	2.00	....
112	.10	.30	.15	....	.30	....	.15	....	.20	2.25	....

No.	CuO	CaO	K_2O	Na_2O	PbO	BaO	ZnO	MgO	Al_2O_3	SiO_2	B_2O_3
113	.10	.30	.15	....	.30	....	.15	....	.20	2.50	....
114	.10	.30	.15	....	.30	....	.15	....	.20	2.75	....
115	.10	.30	.15	....	.30	....	.15	....	.20	3.00	....
116	.10	.30	.15	....	.30	....	.15	....	.20	3.25	....
117	.10	.30	.15	....	.30	....	.15	....	.20	3.50	....
118	.10	.30	.15	....	.30	....	.15	....	.20	1.75	....
119	.10	.30	.15	....	.30	....	.15	....	.20	1.70	.05
120	.10	.30	.15	....	.30	....	.15	....	.20	1.65	.10
121	.10	.30	.15	....	.30	....	.15	....	.20	1.60	.15
122	.10	.30	.15	....	.30	....	.15	....	.20	1.55	.20
123	.10	.30	.15	....	.30	....	.15	....	.20	1.50	.25
124	.10	.30	.15	....	.30	....	.15	....	.20	1.45	.30
125	.10	.30	.15	....	.30	....	.15	....	.20	1.40	.35
126	.10	.30	.15	....	.30		.15	....	.20	1.35	.40
127	.10	.30	.15	....	.30	....	.15	....	.20	1.30	.45
128	.10	.30	.15	....	.30	....	.15	....	.20	1.25	.50
129	.10	.30	.15	...	.30	....	.15	....	.10	1.60	....
130	.10	.30	.15	....	.30	....	.15	....	.15	1.60	....
131	.10	.30	.15	...	.30	....	.15	....	.20	1.60	....
132	.10	.30	.15	....	.30	....	.15	....	.25	1.60	....
133	.10	.30	.15	....	.30	....	.15	....	.30	1.60	....
134	.10	.30	.15	....	.30	....	.15	....	.35	1.60	....
135	.10	.30	.15	....	.30	....	.15	....	.40	1.60	....
136	.10	30	.15	....	.30	....	.15	....	.45	1.60	....
137	.10	.30	.15	....	.30	....	.15	....	.50	1.60	....

In these experiments the K_2O was obtained from pure carbonate potash, Na_2O from crystal carbonate of soda, PbO and BaO from the carbonates and ZnO and MgO from the oxides. Al_2O_3 was obtained entirely from clay, .05 equivalent being raw clay and the balance calcined clay.

The carbonates of potash, soda and barium, and boric acid were fritted, while the other elements were used without fritting, as was the oxide of copper.

The glazes were all carefully prepared and passed through a brass lawn of 200 meshes to the inch. The tiles were glazed and placed together in the same kiln and fired at cone 02. A second lot was placed in an other kiln and fired at cone 2.

THE RESULTS.

The benefit of these experiments can only be gained by a careful examination of the samples. No words can convey the numberless delicate shading of the tints produced. Nevertheless, the pen can describe a few of the coarser or more obvious distinctions, which are given below:

In glazes 1 to 6, the RO is composed of .45 equivalent of CaO and .45 equivalent of one of the other elements. this being the highest quantity of any of the basic oxides thought advisable in this line of experiments.

No. 3, containing PbO, was the only one showing satisfactory glaze results; it developed a very dark green, both at cone 02 and 2, the former having considerable scum over the surface, with well defined crystals around the edges, while the latter was a very clear glaze with a few small crystalline spots on the surface.

The other glazes in this series were immature at both cone 02 and 2, each base, however, showing different action in developing the copper, as well as fusibility. K_2O gave a yellowish green, while the Na_2O leaned to the gray; BaO developed in its semi-vitreous state at cone 02 a turquois green, but lost the color at the higher heat, changing to a blackish green; the ZnO at the lower heat had a chrome green shade, changing at cone 2 to a darker color. Their fusibility was in the order named: PbO, Na_2O, ZnO, K_2O and BaO; the MgO was infusible, the CuO having passed

into the body, leaving the glaze without any color and peeled from the body.

The details of this series has been given more fully than will be possible with the others, to show the action of the different basic oxides when in combination with CaO, and comparison when combined with two or more of the basic oxides.

In glazes 7 to 11 where the K_2O has been reduced to .30 equivalents, and combined with .15 equivalents of one of the other bases, the introduction of the third base gives an entirely different color effect. The zinc glaze, No. 10, is the only one showing a fully developed glaze at cone 2. It had a light bluish iridescence, while at cone 02 it was more of a chrome shade with soft velvety matt effect.

BaO, No. 9, gives a grayish green matt glaze at cone 2, while PbO, No. 8, is a dark blackish green color, iridescent crystaline surface with apparently a clear glaze underneath at cone 2. MgO, No. 11, shows some fusibility at cone 2, with good color effect.

In the glazes 12 to 16, where the .30 equivalents K_2O has been replaced by Na_2O, all show more fusibility than the former series. ZnO, No. 15, is, however, the only clear glaze at cone 2, as it was in the previous series, and a light sea green shade; at cone 02 a dark brown matt glaze was developed.

By the introduction of .30 equivalents of PbO in glazes 17 to 21, a line of more fusible glazes are obtained, with entirely different results from the preceding experiments. The ZnO member—No. 20—is a beautiful clear green, the K_2O shows most fusibility, and the Na_2O has many small crystals on the surface.

The BaO glaze, No. 19, is covered with a scum, showing some tendency to crystalize and is the only one in this series that shows any crazing. At cone 02 this series is thoroughly fused, but the heat is not sufficient to fully develop the glazes.

There is very little variation in the color effect in this series; the increased content of PbO seems to negative the

action of the other bases on copper which is very noticeable in comparing No. 18 with No. 8, or No. 17 with No. 13.

BaO in No. 22 to No. 26, does not give the fusible qualities expected from it, as none of the glazes have cleared. K_2O in No. 24 seems to excite the fluxing qualities of BaO more than the other bases.

In the series containing .30 equivalents ZnO, No. 27 to No. 31, the one with BaO, No. 27, is more fusible than the barium zinc combination, No. 25, which contains a larger quantity BaO. The increase of ZnO excites the fluxing, while the reverse is the case with PbO, No. 28, when compared with No. 20. The most fusible of this series is the Na_2O member, No. 20, which gives a very dark clear grass green, deepening and changing the color when compared with No. 15 which is much lighter and more of a turquoise, while the one with K_2O, No. 30, has a bluish cast more of a sea green, with some fine crystals on the surface.

Nos. 27 and 28 at cone 02 are matt glazes, the former showing some signs of crystalizing, while the latter produces a very soft velvety matt surface of a bronze green color. No. 29 at cone 02 gives a semi-matt glaze, of a chrome-green color.

In the glazes No. 31 to No. 36, containing .30 equivalents MgO, there is no signs of fusion at cone 02, the glaze cracking and peeling from the body, while at cone 2, No. 34, with PbO, is vitrified with dull surface, No. 36 and No. 35 being next in order of fusibility.

The order of fusibility with .15 equivalent of MgO is No. 21, No. 16, No. 11, No. 26, and No. 31, containing .45 Eq. CaO, and .30 Eq. PbO, Na_2O, K_2O, BaO, and ZnO respectively.

In all of the glazes from No. 1 to 36, containing .45 equivalent CaO, there are none of practical use at cone 02, but several at cone 2; No. 20 being a very good glaze, dark green with slight yellow cast, while at cone 02 there are several excellent dull or matt glazes, as above mentioned.

In considering glazes 37 to 41, you will note the first reduction in CaO content to .30 equivalent, dividing the other bases into .30 equivalent each.

A reduction of CaO and the addition of .15 equivalent of one of the other bases, when compared with series No. 7 to No. 11 shows increased fusibility, except in No. 41, MgO; No. 40 containing ZnO gives a bluish opaque color at cone 2 while at cone 02 a semi-mat blue-green glaze. In No. 39 we have a peculiar black-green matt surface at both cone 2 and 02, with very fine crystals covering the surface at cone 2.

In the soda Series, No. 42 to No. 45, there is an increased fusibility when compared with No. 12 to No. 16, with the exception of No. 45, MgO. The whole series is more fusible than the one containing K_2O. The color in No. 44 at cone 02 gives a gray-green matt surface and loses the blue color produced by K_2O in No. 40.

In Nos. 46 to 51, which closes the .30 equivalent series, are some interesting effects. In No. 47 the increase of zinc does not perceptibly affect the color or fusibility in comparison with No. 20, except that in the former, small floculent spots appear suspended in the glaze. No. 49 has qualities worthy of further investigation, producing a semi-glossy surface of very good dark green. Nos. 48 to 51 are matt surfaces, the first a yellow-green, while the other is a gray-green.

In the next series of experiments, a fourth element is introduced in the RO, with increased fusibility and change of color all through the series; but I will not attempt to give a detailed analysis, only calling your attention to a few of the most marked changes. Again, the larger content of Na_2O over K_2O shows in a marked degree more fusibility and decided difference in color effect, as seen by comparing No. 53 and No. 54 with No. 68 and No. 70 at cone 2.

Nos. 58, 59, 61, 69 and 78B at cone 2 are dull on semi-dull surfaces, the colors being entirely different, varying from a gray to a blue and brownish-green. The surface of No. 69 is covered with fine gray needle crystals, with a darker green in the body glaze, and is worthy of further consideration as are all the others.

The Series No. 70 to No. 82, containing .30 equivalents

of barium produces at cone 2, some dull effects that can be followed to advantage in some further experiments.

No. 85 is a fully developed, very dark brilliant green glaze at cone 2, while at cone 02 shows little signs of fusion.

The first combination with MgO, giving satisfactory results, is the one with lead and zinc, No. 76, giving a dark, clear green glaze, but practically the same color as the ones with K_2O and Na_2O content, No. 77 and No. 78, indicating that MgO has no peculiar action in producing copper greens.

In the series containing an equal division of the RO elements, the results were not as anticipated, as more fusible glazes were expected. The only ones showing any tendency to a clear glaze, were Nos. 91, 92, 94 and 96, at cone 2, No. 94 being a fully developed dark green glaze. No. 97, the only one in the entire series without CaO, gives every indication of a good glaze at a little higher heat, and does not indicate that lime has any decided effect in developing copper greens.

In Nos. 98 to 106 is an increase of Al_2O_3 with corresponding increase of SiO_2, keeping the bi-oxygen ratio, beginning at a very low SiO_2 content without any Al_2O_3 and increasing by .05 equivalent up to .40 equivalent, and in Nos. 129 to 137 is an increase of Al_2O_3, without any change in SiO_2 content, reducing from an oxygen ratio of 2.46 to 1.28.

The effect of an increase of Al_2O_3 is very marked, the color gradually changing from a bright emerald green to a very deep green in No. 101 and then gradually getting lighter to a yellow green with fairly satisfactory glazes at cone 2 up to the one containing .30 equivalent of Al_2O_3, No. 105, and at cone 02, up to .20 equivalent Al_2O_3, No. 102, with very slight variation of color between the two temperatures on the dust pressed body, while on the earthenware body at cone 02, the glazes are better developed, but show a tendency to crazing.

No. 98 without any alumina, is a very good glaze at cone 2, while at cone 02, small crystals are seen along the

edges and on the surface of the glaze; No. 106 begins to show a flaky crystalline effect from the increase of Al_2O_3.

The same effect of producing a yellow green is also very noticeable in Series Nos. 129 to 137, giving a dark green up to No. 131, when the yellow begins to show its effect on the color, by the increase of Al_2O_3, but not so much as in the other series; .10 equivalent at cone 02 and .25 equivalent at cone 2 seems to be the limit of Al_2O_3 in producing glazes with SiO_2 content of 1.60 equivalent. The glazes beyond these qualities having a matt effect with a soft velvety surface on the higher Al_2O_3 content at cone 02 and cone 2; .40 equivalent produces the best mat surface at cone 02 and .50 equivalent at cone 2.

This line of experiments on the earthenware body are more fusible, without any change in color.

With the increase of silica, maintaining alumina constant, Series Nos. 107 to 117, we have good glazes from Nos. 108 to 113 at cone 2, and fairly good at cone 02, the latter showing some tendency to creep, which no doubt can be corrected by a mechanical change in the mixing of the glaze. The higher silica glazes of this series are in various stages of devitrification, the first indication appearing in No. 111, but all of them produce a rough texture.

There is a decided difference between Nos. 107 and 108, .25 equivalent of SiO_2 changing the glaze from a matt to practically a clear glaze at cone 02, and at cone 2 to a beautiful shade of green with a little tendency to crazing. No. 107 at cone 2 has lost the matt, changing into a fully vitrified glaze, covered with small crystals. The texture of the matt effect in No. 107 is very much the same as produced by high Al_2O_3 content, and compares very closely to No. 133 in surface, but not in color. Increased SiO_2 content has no perceptable change in color at either temperature.

The earthenware body in this series gives about the same degree of fusion at cone 02 as the dust pressed body does at cone 2, but no difference in the color.

The boric acid series, Nos. 119 to 128, gives a good line of glazes at cone 2 with very little change in color, but showing some signs of bleaching to a lighter color; but at

cone 02 on the dust pressed body, the lower series of B_2O_3 are somewhat crystalized and looks very much like the beginning of divitrification, decreasing as the B_2O_3 increases; Nos. 122 to 128 are clear glazes, the higher content giving a brighter green. On the earthenware body, the glazes are all good, but there is considerable change in color between cones 02 and 2, the latter giving an entirely different quality of green.

The separating and creeping of many of these glazes, can be corrected by changing the mechanical mixing, either in the glaze or the making of the fritt, but in this line of experiments a uniform method was used to get exact comparison of the peculiar action of the RO elements when in combination with oxide copper.

The deductions to be derived from this line of experiments are such as to excite the desire for a further investigation, and makes me hesitate to place before you what I consider is incomplete; and in doing so, I sincerely hope my results will be the means of starting some one on a similar and more complete analysis of the development of greens from cupric oxide. I have not had the time to verify any of these experiments, and give them to you merely as first results.

The alumina content of .20 equivalent in Nos. 1 to 97 was decided upon, after several preliminary trials, as the best content for this investigation. A different proportion might have changed some of the results, producing more satisfactory glazes, but without giving the same comparison.

From these experiments, the indications are that CaO can be used in any quantity found necessary in producing a satisfactory glaze, without affecting the color produced from copper. But with the introduction of K_2O, the color is changed to a yellowish tint; and and when in combination with either Na_2O or PbO, the K_2O can not exceed .15 equivalent without affecting the color; but ZnO controls the green color when content of K_2O is as high as .30 equivalent: the color becomes very much deeper as the K_2O decreases and the ZnO is increased. This same effect of

deepening the color is produced when ZnO is combined with Na_2O, but with an entire change of color.

The same can be said of the zinc-barium glaze, the color getting stronger when the ZnO is increased to .30 equivalents and BaO decreased to .15 equivalents. BaO is more fusible when in combination with ZnO than with Na_2O, producing a better working glaze. The barium-zinc glaze has decidedly a blue shade when combined with either Na_2O or K_2O, but with PbO has more of a chrome green color. BaO when with .15 equivalents of MgO produces a color very different from that produced with PbO.

ZnO when in combination with either K_2O or Na_2O and not exceeding .15 equivalents gives a light clear glaze leaning to the blue; but when the ZnO is increased to .30 equivalents, and the CaO reduced to .30 equivalents, the one with K_2O produces a lighter shade, decidedly blue and opaque; while the one with Na_2O changes color more to the brown, but gives a clear glaze.

ZnO has a very decided action in producing colors in both clear and matt glazes; .15 equivalent shows a more marked effect than .15 of any of the other bases. ZnO can be used to advantage with all basic oxides, making the color more brilliant when in combination with PbO, and giving most beautiful and varied shades from copper when in other combinations. I am not prepared to say conclusively that ZnO has a strong tendency to excite the fluxing with cupric oxide, but every indication points to such action.

.30 equivalent of PbO when in combination with .45 equivalents of CaO and .15 equivalents of one of the other bases, controls completely the color, producing a clear deep green; but loses this effect when reduced to .15 equivalents: with the reduction of the CaO to .30 equivalents, the PbO does not have the same power, the color being modified according to the other base introduced.

Replacement of CaO with MgO retards fusion, without changing the color, except in the one with PbO, which is a very much lighter shade of green; and more of a gray-green when the MgO is increased.

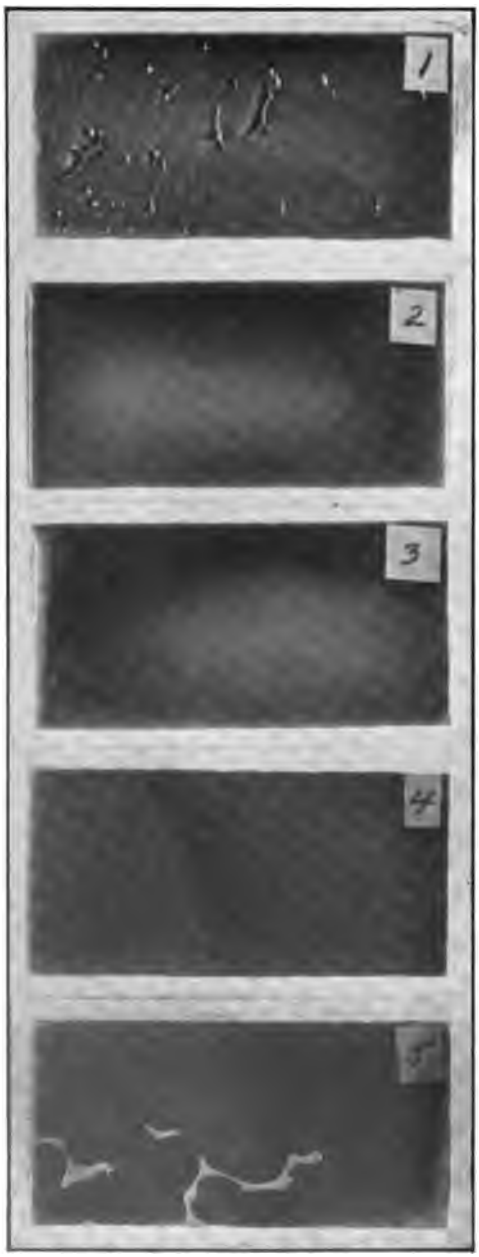
With the introduction of boric acid, replacing equiva-

lent contents of silica, we have a line of colors showing but slight changes, the largest B_2O_3 content being a little lighter but more brilliant green; while the SiO_2 content in all proportions seem to give about the same shade; but with alumina the content must be kept down to secure a pure green color.

In summing up the facts demonstrated from these experiments, SiO_2 and B_2O_3 can be used in any desired content, while Al_2O_3 should not exceed .25 equivalents, unless the shade of green desired is to be of yellow tint; while in the RO elements, not over .30 equivalents should be used of any one base for best effects.

I have endeavored to deduce something from these experiments that will prove interesting and of advantage, but fully realize my lack of descriptive power to convey intelligently to you the color changes. I am fully convinced there is but on way for you to get any real benefit from this paper, and that is for you to make a similar line of experiments, and I certainly cannot urge you too strongly to do so, assuring you that the labor connected therewith will be fully repaid by the interesting study you will have when completed.

NOTE BY THE EDITOR:—The effort has been made in the following pages to reproduce the color series which Mr. Walker exhibited while reading his paper. The effort has been only partly successful, as it only shows the color effects by variations of shade, and cannot bring out the marvelous differences of quality of color. However, the plates in connection with the descriptive text will help the careful student to gain some impressions which would otherwise be wholly lost.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Series Fired at Cone 2.

1. *Composition.* 0.10 CuO 0.45 CaO 0.45 K_2O, 0.20 Al_2O_3 1.60 SiO_2.

An imperfectly fused, matt glaze, with signs of glossy surface around edges. Not fluid enough to heal over the defects formed during shrinkage. Color, dirty yellowish-green around edges, which show dark in the cut. In the center, the color shades into a steel gray, which reflects light and appears lighter in the cut. No promise for any practical purpose.

2. *Composition.* 0.10 CuO 0.45 CaO 0.45 Na_2O, 0.20 Al_2O_3 1.60 SiO_2.

A much better fused glaze, smooth to the touch. It is not a good matt, but is equally removed from a glossy surface. Shows few lines, indicating where shrinkage cracks formed and subsequently mended. Color, apple-green around edges, which show dark in cut, but shading into a dark grayish-black color in center, which appears lighter in the cut Not attractive.

3. *Composition.* 0.10 CuO 0.45 CaO 0.45 PbO, 0.20 Al_2O_3 1.60 SiO_2.

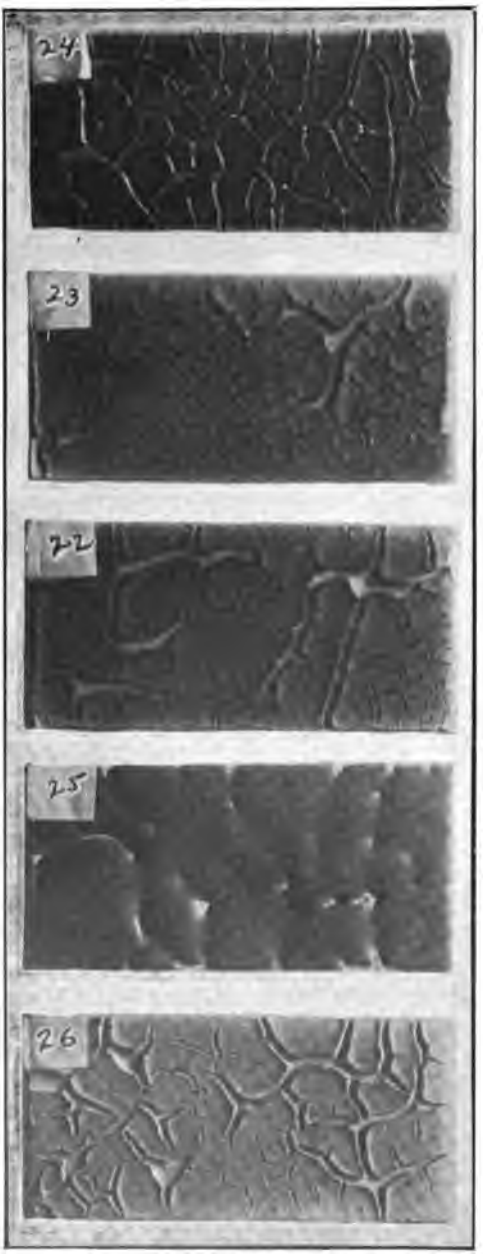
A brilliant perfectly fused glaze of high finish. A few smal blotches of some undissolved matter, seen faintly in the upper left-hand corner of the cut. Color, a brilliant clear copper-green, apparently equally removed from the blue or yellow tints. Contrast is due to the difference in depth of glaze, which makes the color very dense in left-hand upper corner. Could be used, as far as color or surface is concerned, for any purpose.

4. *Composition.* 0.10 CuO 0.45 CaO 0.45 ZuO, 020 Al_2O_3 1.60 SiO_2

A semi-crystalline matt, whose imperfectly obliterated shrinkage lines form a rather coarse net work, giving a peculiar and rather pleasing curdy appearance. Surface feels much less smooth than No. 2 and smoother than No. 1. Color is an opaque blue-green, almost of the tint known as "robin's egg" blue. A glaze which invites further development.

5. *Composition.* 0.10 CuO 0.45 CaO 0.45 ZuO, 0.20 Al_2O_3 1.60 SiO_2.

An imperfectly fused glaze, which has not flowed enough to cover all of its shrinkage cracks, though it has covered some. Surface is matt, mostly, but in a few patches, especially on left hand end, the scum breaks away and exposes a glossy surface. The scum or matt surface gives this tile a grayish-green cast, which appears clear and darker in color where the film is broken away. Not a workable glaze at present.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Series Fired at Cone 02.

0.20 Al_2O_3, 1.60 SiO_2 throughout series.

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24. *Composition.* 0 10 CuO 0.45 CaO 0.30 BaO, 0.15 K_2O.

A crackled, shrunken mass, just beginning to soften and round off the edges and corners. Color, light bluish-green around edges of tile, but a strong dark chrome green in all the central portion of tile.

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23. *Composition.* 0.10 CuO 0.45 CaO 0.30 BaO, 0.15 Na_2O.

A much dryer, less vitreous mass than 24, cracks numerous and edges sharp. Would not have fused for a number of cones higher. Color faded out to a light blue around edges. In central portion, the color is a dark neutral tint, or slate blue, with a slightly purplish cast. No likelihood of this glaze becoming useful

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22. *Composition.* 0.10 CuO 0.45 CaO 0.30 PaO, 0.15 PbO.

A still less fused mass, shrunken and vitrified and crossed by numerous deep, sharp edged cracks. Volatilization has bleached edges to a clear light blue-green, but center is a much darker shade of the same color, not unlike No. 23, but bluer. No utility. This slab is remarkable, in view of the high fluxing value of lead and its small effect here, compared to fluxes which are elsewhere less active.

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25. *Composition.* 0.10 CuO 0.45 CaO 0.30 BaO, 0.15 ZnO.

A smooth, satin-finish mass, imperfectly fluid, not wholly obliterating the shrinkage cracks, through which the body sometimes shows. Texture wonderfully fine. Color, a beautiful dark chrome-green enamel, marked with black lines where cracks have been healed over. If a little more fluid, so as to cover the body wholly, this would make a fascinating surface finish for art ware.

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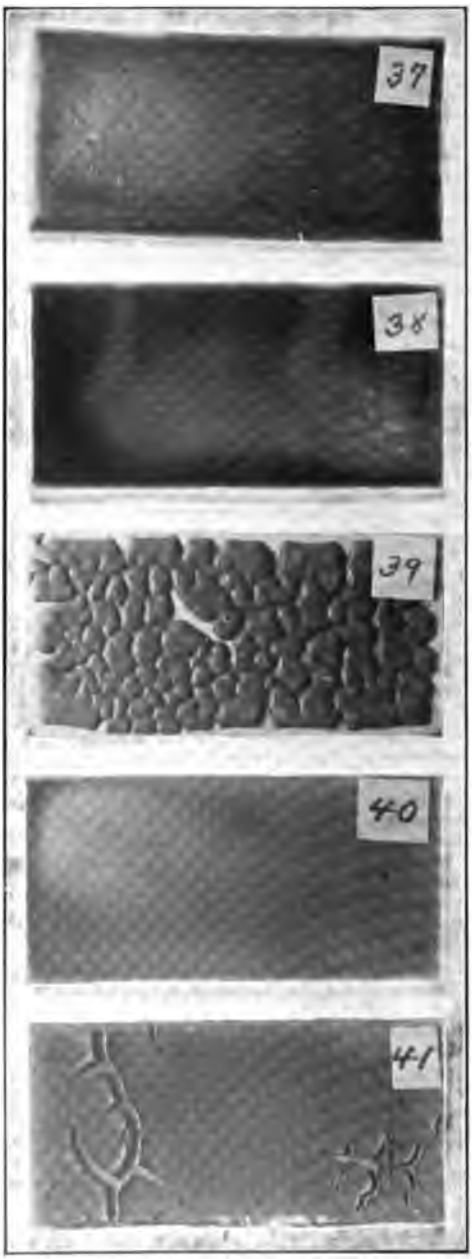
26. *Composition.* 0.10 CuO 0.45 CaO 0.30 BaO, 0.15 MgO.

A dry, imperfectly vitrified substance, shrunken and cracked through to the body, and with edges sharp and rolled upwards somewhat. Color, light blue-green or robin's-egg blue. The absence of green in this color makes the cut show light in tone. No signs of probable utility.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Series Fired at Cone 2.

0.20 Al_2O_3 1.60 SiO_2 throughout Series.

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37. *Composition.* 0.10 CuO 0.30 CaO 0.30 K_2O 0.30 Na_2O.

Fairly well fused, obliterating all shrinkage cracks. Covered with a whitish crystalline scum, which destroys the color of the glaze and makes it appear gray in cut. Around edges, the color is an opaque green with a somewhat bluish tint. No utility.

---

38. *Composition.* 1.10 CuO 0.30 CaO 0.36 K_2O 0.30 PbO.

A brilliant glossy surface, disfigured by crystalline segregations on right hand lower corner. Oily spots or drops in the glaze, give it a dappled effect by reflected light. This does not show in the cut. Color, a pure dark green, like No. 3. Difference in tint in cut due to drainage and thicker glaze at those points. This glaze is feasible for use, but not as good as No. 3.

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39. *Composition.* 0.10 CuO 0.30 CaO 0.30 K_2O 0.30 BaO.

A shrunken, crackled mass, in which the edges have fused and rounded and the cracks healed over in part. Color is rather dull dark bluish-green, not dissimilar to No. 4, which is similar in composition. Glaze not valuable in its present shape, but offers a good lead for investigation.

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40. *Composition.* 0.10 CuO 0.30 CaO 0.30 K_2O 0.30 ZnO.

A smooth, brilliant, glossy glaze, whose surface shows only slight defects in reflected light. Color is very remarkable. Around edges, pale transparent apple-green, showing dark in cut. Then comes a border, one-quarter inch wide, of bright azure blue, semi-transparent and contain fibrous suspended matter, which can be seen in cut. The center is a uniform opaque blue-green or "robin's egg blue." The color seems accidental, and if it could be duplicated might be useful.

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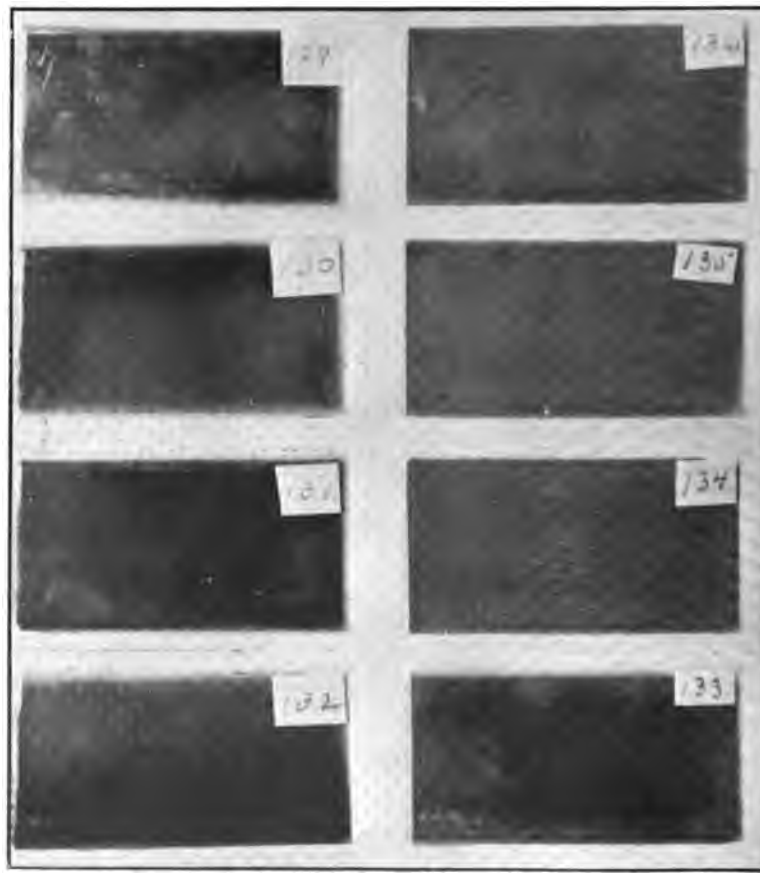
41. *Composition.* 0.10 CuO 0.30 CaO 0.30 K_2O 0.30 Mgo 0.20.

A gray slightly greenish unfused and rough surface with wide shrinkage cracks. No utility.


127
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133

Series Fired at Cone 2.

Formula $\left.\begin{matrix}0.10\ CuO\\0.80\ CaO\\0.15\ K_2O\\0.30\ CaO\\0.15\ ZnO\end{matrix}\right\} x\ Al_2O_3.\ \ 1.60\ SiO_2$

129. $Al_2O_3 = 0.10$
A fine brilliant copper-green glaze, perfectly transparent and free from blemishes, except slight undissolved matter along lower edge.

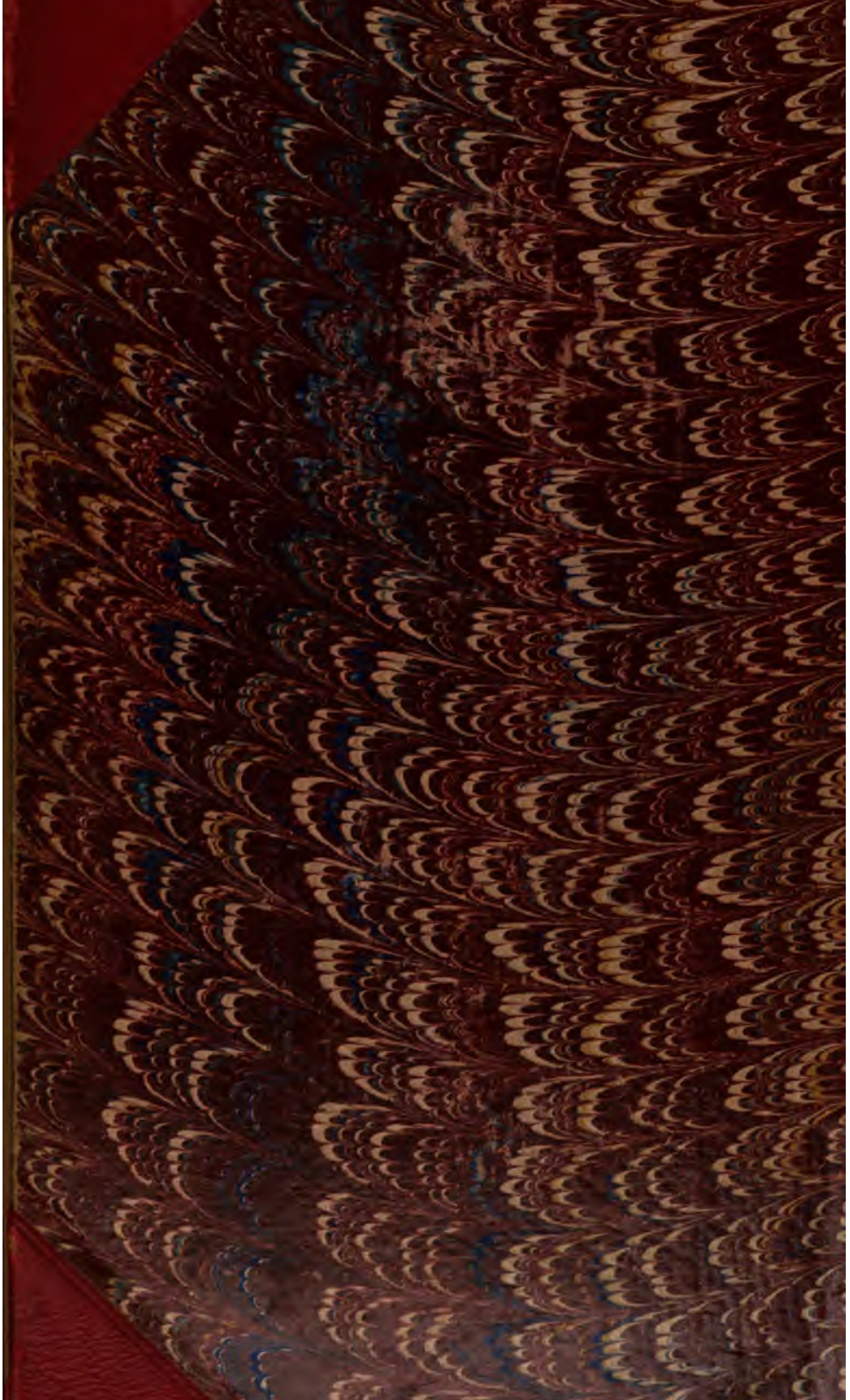
130. $Al_2O_3 = 0.15$
Same as above, without the undissolved matter.

131 $Al_2O_3 = 0.20$
Darker, yellower green. Glaze was probably dipped heavier than preceding, but color is also different.

132. $Al_2O_3 = 0.25$
About like 131, only the glaze has drained to lower edge, perfectly transparent.

133. $Al_2O_3 = 0.30$
A yellower green, semi-opaque and with a good deal of a whitish segregation around edges and left hand end. The surface still bright shows dark in the cut.

134. $Al_2O_3 = 0.35$
A beautiful matt glaze, smooth as silk, with a delicate whitish dapple mark. This glaze is very promising for general matt purposes.

135. $Al_2O_3 = 0.40$
A somewhat lighter colored but equally beautiful matt glaze, very soft and silky in texture. Dapple marks are much finer, but still distinguished in cut. As good as 134 for use.

136. $Al_2O_3 = 0.45$
A paler green than any. Dapple marks very fine indeed. Glaze is still smooth and perfect for a matt, but the marks of recently healed shrinkage cracks show that the glaze is near its workable limit.